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### Experiments on the Paranormal Cognition of Drawings

WHATELY CARINGTON

#### III

### Steps in the Development of a Repeatable Technique

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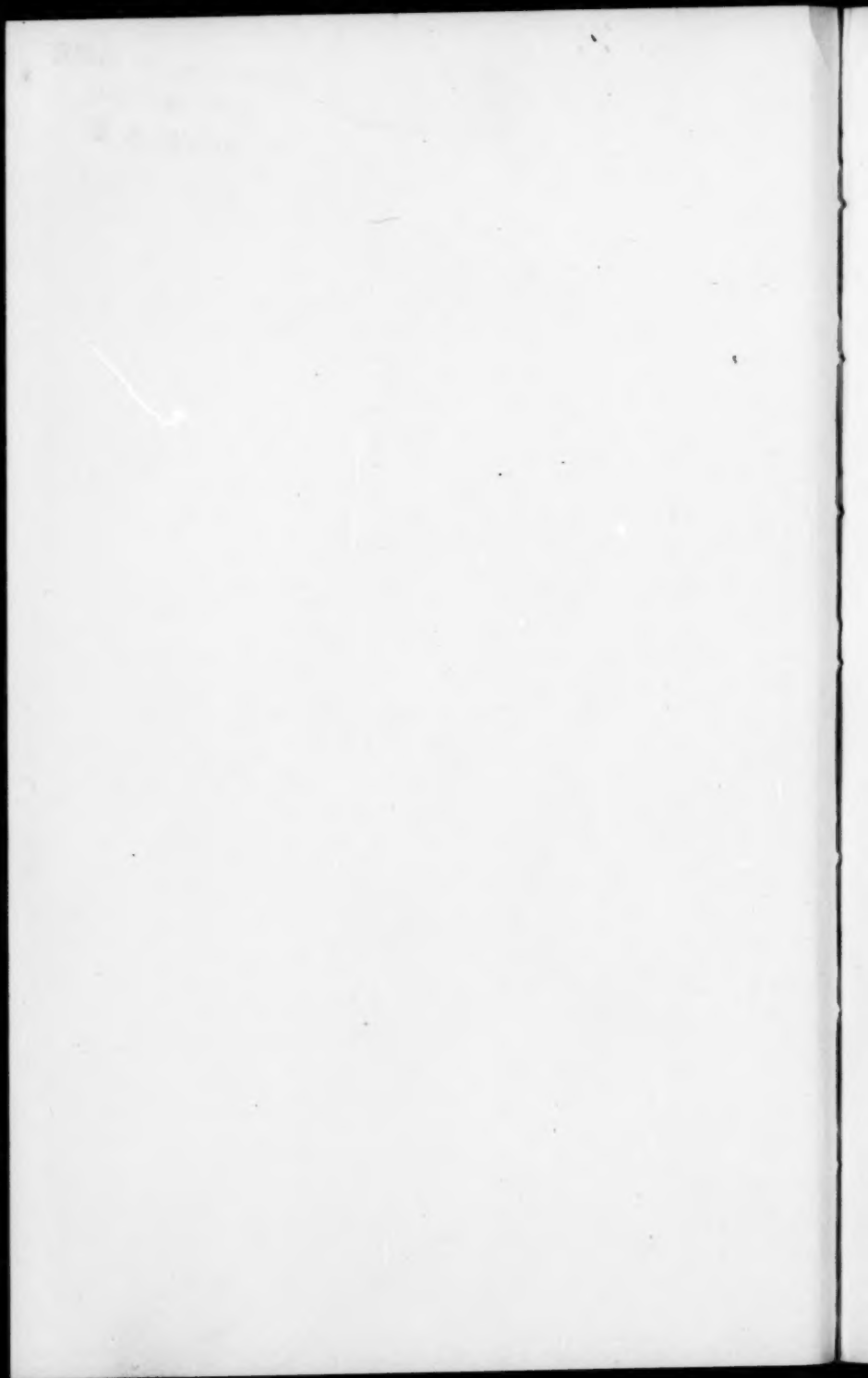
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Experiments  
on the  
Paranormal Cognition of Drawings

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WHATELY CARINGTON

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III

Steps in the Development of a Repeatable Technique

Part I. Introduction: Demerits of Cross-Scoring: Use  
of 'Fisher Scores': Example

1

In the course of my first paper on this subject (1) I had occasion to point out that "the repeatable experiment is the very foundation of science," and I went so far as to say (p. 128) that, whatever opinions might be expressed about the work, it was "by the test of repeatability alone that it must ultimately stand or fall." Since then, I have become increasingly convinced of the soundness of these remarks; and I wish to begin the present contribution by emphasizing the importance of the point of view then expressed, and of the desirability of devising means for giving effect to it in practice.

The essential points I want to make may be put as follows: Colloquial convenience apart, it is strictly impossible to 'prove' the genuineness of a newly discovered phenomenon in the sense that we can 'prove' the truth of a mathematical theorem; the second is a matter of deductive reasoning, where all valid conclusions are already implicit in the definitions with which we start, while the first has more of the character of inductive inference. We can, it is true, often apply deduction in an established field, and declare that certain phenomena, not yet observed, are likely or virtually certain to occur; but we can only do this *after* the 'laws' governing that field have been inferred from observation of the relevant phenomena: we cannot use it to establish the phenomena in the first instance.

It follows, I think, that we can never hope to attain finality, as regards general acceptance of paranormal cognition or the like, by any process, however far extended, of merely elaborating and stiffening particular researches or techniques, in the hope of achieving an unassailable rigidity. This is strictly impossible, for the hostile critic can always produce *some* alternative hypothesis, if he is willing to

make it elaborate enough—somewhat as we can always fit a line to any number of points if we care to use a sufficiently complicated equation—and in the last resort he can always postulate collusive deception to any required extent. And if we protest that his counter-hypotheses are implausible and his postulations offensive, he will merely aver that paranormal cognition is more so; and this at once shifts the discussion out of the realm of logic into that of opinion, in which no coercive conclusion can be reached. Once the parapsychologist allows the critic to start the game of chivvying him from implausible pillar to preposterous post, he has more or less foredoomed himself to defeat, for he has tacitly taken up the challenge to perform the impossible task of designing a logically invulnerable experiment.

## 2

Nothing that I have said above is to be taken as implying that any reasonable precaution should be relaxed, or any proper care remitted, in the conduct of experiments, or that we should not do our best to make them proof against even unreasonable criticism; my point is that it is simply not possible to bring opposition (in which I do not include constructive criticism, which should always be encouraged) to a final end by these means.

It would indeed be contrary to all the lessons of scientific history to suppose that one could. Almost every discoverer of new and surprising facts, not fitting neatly into existing preconceptions, has encountered opposition, of which the intensity has usually depended less on the merits of the case than on the degree of emotional resistance aroused. This opposition has seldom if ever been overcome by arguing about the validity in detail of this particular experiment or that, but almost always by devising means whereby anyone who likes to take a reasonable amount of trouble may observe the disputed facts for himself. Science, in fact, is essentially a public and social activity, to which anything savoring of the esoteric is very rightly anathema, so that scientists have naturally looked askance at claims that cannot be substantiated in open court.

Thus, although in the earliest stages it is inevitable that pioneers should plough lonely furrows, it is in the highest degree desirable in the public interest that we should, as soon as possible, put on the market, so to say, a serviceable instrument for general use. If we can do this, we may reasonably hope that there will not be lacking persons of enterprise and integrity willing to put the instrument to use and report their results.

I personally regard this as of much greater long-term importance—provided there is a reasonable prospect of success—than attempting to press on, each by himself, towards new discoveries. Experience in other fields clearly indicates that, once we have twenty or thirty professional psychologists, or other persons of competence and repute, doing experiments (whether with drawings or other-

wise) as a matter of routine research, and obtaining positive results with regularity, the discoveries will not be long delayed. A good repeatable technique is, indeed, to science what a machine tool is to industry; with it we can go at once into profitable production on a commercial scale, as it were; without it everything must be laboriously made by hand.

I have accordingly devoted the last twenty months, or thereabouts (I write in March, 1942), almost exclusively to the attempt to develop and try out a technique for this purpose, with the results to be described below.

3

It would be unreasonable to expect busy men, with problems of their own, to undertake experiments in this field, if the suggested technique failed to satisfy certain conditions, which it is worth spending a few moments in discussing.

The two basic requirements clearly are, first, that it should be *valid* and, second, that it should be *practicable*. By the first I mean that, given a normal degree of care, intelligence, and integrity, such as we may reasonably postulate in any responsible worker, it should be virtually impossible to obtain spurious results; by the second, that it should not make prohibitive demands in the way of unobtainable apparatus, special skill or knowledge, inaccessible 'sensitives,' or excessive expenditure of time and energy.

I might, perhaps, have added a third requirement, namely that it should 'work'—i.e., be capable of successfully demonstrating the phenomena in question if they be present, though this more or less goes without saying. In this connection, however, it seems worth while to point out that the term 'repeatable' does not mean 'invariably successful.' Experiments in chemistry and physics, if properly conducted, yield 'invariable' results, but only because the numbers of atoms, etc., involved are so large that the chance of detectable aberrations occurring is infinitesimally small. In the biological sciences, on the other hand, and especially in psychology, one might fairly say that invariability of result is the exception rather than the rule; and we do not deny the validity of a medical treatment which reduces mortality from 70% to 50%, without saving the life of every patient. The requirement in these fields is simply that it should be possible to attain statistical significance in a not undue number of trials.

These, then, are the characteristics of a satisfactory technique, and I believe that the methods described below meet them sufficiently for all practical purposes at the present stage. But I am anxious not to suggest or imply that I regard these methods as perfect or final. Science is more of an organic growth, rooted in trial and error and nourished by the contributions of innumerable workers, than a body of logically impeccable propositions excogitated by purely intellectual activity. Scientific techniques are seldom born fully-armed into perfection, like Athene from the head of Zeus; they are most usually

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evolved by degrees through the elimination of defects and the addition of improvements from multifarious sources. Thus it would be a mistake to regard this paper as in the nature of a statement of claims concerning a finished product; it is rather to be taken as a narrative report on what I have done, and why, and what came of it; and as an invitation to others to conduct experiments on the same lines and see whether they obtain similar results.

4

The most convenient way of developing the discussion will be by briefly reviewing my original methods and noting the respects in which they failed to meet satisfactorily the requirements mentioned above. For the benefit of those who are unfamiliar with my first paper the procedure adopted may be shortly recapitulated as follows:

In each experiment, the experimenter randomly selected a series of ten words from a dictionary, and illustrated each by a simple drawing known as an 'original.' In four experiments out of five, a different original was thus produced on each of ten successive evenings; in the second experiment, at about five minute intervals in the course of an hour or so. The percipients, varying in number from 11 to 105, and so located that they could not possibly see what the original represented, attempted to 'reproduce' what was drawn. The fifty originals arranged in a relevantly random (actually alphabetical) order, together with all percipients' drawings (not randomized) were then passed to an external judge, who knew nothing whatever about which original had been used in which experiment. This judge was instructed to 'score' all drawings against all fifty originals, giving a full point ('hit') wherever a drawing plainly represented the same object as the original, and a half point in doubtful cases. The numbers of hits thus scored on the originals of each experiment by the percipients of each experiment were then arranged in a 5 x 5 table, and the excess over expectation of the number of hits scored by percipients on the originals used in the experiment in which they were engaged (regardless of its occasions within the experiment in which they were drawn) was assessed by Stevens' method. Highly significant results were obtained.

5

From the point of view of validity, there is very little wrong with this procedure, though I shall have something to say, in sections 38 and 39 below, about a point of some theoretical interest. Strictly speaking, it would have been better to have randomized the drawings as well as the originals; but I have dealt with this in my second paper (2. pp. 342-345), pointing out that it could not affect cases (which by themselves give a significant result) in which there could be no question as to whether a hit should be scored or not. The dictionary method of selecting originals has certain serious drawbacks from the practical point of view, but its relevance to validity

is negligible (Cf. 2, p. 340). As for the nominally all-important point of preventing percipients from obtaining normal knowledge of the originals, this is so easily secured by having the experimenter always in a different room or building from them that it need not be further discussed. It is worth noting, however, first, that my experiments have so far shown no need whatever to pander to any feeling percipients may have that it is 'easier' to get good results if they are in the same room as the experimenter, or close to him; second, that percipients working together in the same room should be invigilated if they cannot be trusted not to 'crib' each others' efforts—otherwise there may be a tendency for the magnitude of the result, whether positive or negative, to be in some degree inflated.

The validity of the actual mathematical treatment used is also not in doubt; the only possible question would be as to whether the data to which it is applied conform to the conditions it assumes, and this will form part of the discussion in sections 38 and 39.

6

As regards the requirement of practicability, on the other hand, the procedure leaves a good deal to be desired.

There is, of course, nothing in principle to prevent anyone who chooses from conducting a series of experiments involving 250 percipients and extending over a period of some six months; but the number of people able and willing to undertake so considerable a labor is likely in practice to be negligibly small. Similarly, there is no theoretical limit to the number of reasonably intelligent and suitably ignorant judges who could be found capable of scoring two or three thousand drawings against fifty originals, but the practical difficulty of finding them is likely to be very great. Moreover—and this is important—whenever a fresh experiment is undertaken, a fresh judge must be found, and either the whole of the previous material, or some considerable part thereof, must be rescored; for otherwise we should have no standard by which to assess the new. The grave practical disadvantages of this are too obvious to need elaboration here. Further, unless judges are bound down to an absolutely rigid literalism by the instructions given them—in which case the procedure becomes so objective that their services might as well be dispensed with—no two will adopt quite the same policy or standards in doubtful cases, so that the experimenter may well find himself in doubt, in certain circumstances, as to which of two or more scorings by different judges to use for particular purposes.

7

Another disadvantage, in no way invalidatory and equally not to be classed under the head of impracticability, but scarcely the less serious in practice, arises from the fact that by pooling all hits on the originals of a given experiment we are automatically treating them as of equal value; this is manifestly contrary to fact, and its effect is seriously to blunt the sensitivity of the method. In the

second experiment, for example, one of the originals used was SPINNING TOP, while another was HORSE; and of the 20 percipients engaged two scored hits on the top and four on the horse, which were entered in the 5 x 5 table as six undifferentiated hits. But of the 721 percipients taking part in the other six experiments of my first seven, 74 drew horses, while only 6 drew tops; thus, when neither of these objects is used as an original, i.e., under substantially chance conditions, one is drawn about twelve times as often as the other. It is repugnant to common sense to credit percipients no more for scoring hits on the rare TOP than on the common HORSE; and, in so far as, on any system of assessment, failure to draw an object used as an original so often as would be expected under chance conditions is bound to involve some kind of 'penalty,' so to say, tending to yield a negative score or to diminish a positive, it would be correspondingly improper to penalize them no more for failing to draw common than rare originals.

I have no doubt that this kind of thing would tend to even out in the long run, and whether on the whole it tends to work for or against the phenomena we are studying will presumably depend on whether common or rare objects are being drawn relatively more often than chance expectation would indicate. But not every experimenter will be able to work on a large enough scale for this evening-out process to operate, while it is easy to show that this failure to distinguish the differing values of hits may very seriously handicap individual experiments; and, after all, it is almost as important that the technique should give the phenomenon a fair chance of showing itself if present as that it should not falsely indicate it if absent.

I think it is worth while to consider an imaginary example of this: Imagine the somewhat extreme case of two experiments each involving 20 percipients but only two originals, and suppose that these were HOUSE and OILCAN for expt. A and APPLE and RAZOR for expt. B. Now experience shows that when these objects are not used as originals the empirical probabilities of their being drawn by a percipient making 10 drawings are approximately .190 for HOUSE, .0027 for OILCAN, .142 for APPLE and .00135 for RAZOR. Then for 20 percipients making two drawings each the expectations will be about .76, .01, .57 and .0054 in the four cases respectively. Suppose now that the actual hits scored were: By the 20 percipients of A, 1 HOUSE, 1 OILCAN, 2 APPLES, no RAZOR; and by those of B, 2 HOUSES, no OILCAN, 1 APPLE, 1 RAZOR. Obviously there is nothing very exciting about the A's having drawn 2 APPLES, or the B's two HOUSES, while the former have pulled off a .01 shot on OILCAN and the latter a .0054 shot on RAZOR, as the best of two trials in each case. Each is intrinsically significant; yet if the data had been treated by the 'cross-scoring' method of the original procedure they would have yielded an exactly null result, for each group scores two hits on its own originals and two on those of the other group.

Although we are interested primarily in the hits scored by percipients on the originals at which they are aiming, and not in those



on originals at which they are not aiming, we are compelled in any such case as this to take cognizance of the latter (possibly, as here, to the considerable detriment of the result), because with this procedure each experiment is the only basis we have for assessing the probability that the originals of the other will be drawn when not used as originals.

Another drawback is that in the case where two experiments are cross-scored against each other, as it would be natural for the independent small experimenter to do, it is impossible to say (since there is only one degree of freedom) which of the two is responsible, or the more responsible, for whatever result may be obtained even though common sense might irresistibly suggest that one showed a much finer performance than the other; indeed, such a question is as meaningless as to ask whether A is more taller than B than B is shorter than A. It is accordingly futile to attempt to ascertain which of two conditions, or groups of percipients, gives the better result by merely doing two experiments and cross-scoring them; and this is a serious handicap from the research point of view.

Finally, the results of cross-scorings, which are necessarily in the form of a deviation from expectation divided by a standard error, do not lend themselves at all well to the manipulations which informative research requires.

In short, quite apart from the difficulty of finding judges, and from their possible idiosyncrasies, the procedure of cross-scoring, though unavoidable in the earliest stages of the work, is cumbrous, insensitive, and almost incapable of yielding the type of information most needed.

## 8

What is wanted is clearly a method whereby the result of any isolated experiment can be quantitatively determined by the experimenter himself, without undue labor, as soon as the work is done; and one, moreover, in which due weight is given to the varying values of hits on rarely and commonly drawn objects.

I think there is only one way of meeting these demands, and of this I shall now give some account.

Discarding altogether the cross-comparison of local experiments, I substitute negative-logarithm *scores* of the kind first introduced by Dr. (now Professor) Fisher in 1924 for the purpose of assessing partial successes in guessing playing-cards (3), coupled with the use of a Catalogue, presented in Appendix I, of the frequencies of all objects drawn or mentioned by the 741 percipients of my first seven experiments—that is to say, all the material that has been indexed up to the time of writing.

I shall have a great deal to say about the Catalogue in later parts of this paper, so it will be convenient to take it for granted for the moment and to consider other aspects of the method before discussing it in detail.

In any experiment, the 'Fisher score,'  $f$ , for any original is given by the expression  $-(h-np) \log p$  or, as I think it more conveniently written,  $(h-np) \log \frac{1}{p}$ . This has variance  $npq \log^2 \frac{1}{p}$ . For the experiment as a whole, the Score,  $F$ , is the sum of the scores for the different originals, and the Variance,  $V$ , the sum of the corresponding variances. In these expressions  $n$  is the number of percipients taking part in the experiment to be assessed,  $h$  is the number of them making hits on the original under consideration<sup>1</sup>, and  $p$  is the empirical probability, determined with the aid of the Catalogue, of a percipient making a hit on that original. As usual,  $q$  is  $1-p$ . On the basis of these definitions, it would seem natural to take  $p$  as  $c/N$ , where  $c$  is the number, as given by the Catalogue, of percipients who have drawn the object in question in the course of experiments in which that object was *not* used as an original, and  $N$  is the total number of percipients working in such experiments. Obviously, if the object was not used as an original in any of the first seven experiments,  $N = 741$ .

This would be correct if the number of percipients dealt with in the Catalogue were indefinitely large (corresponding to a 'theoretical' value for  $p$ ) so that every object that could possibly be drawn was included; or if we agreed to confine our selection of originals rigidly to objects listed in the Catalogue. But the first proviso does not obtain, while, although the second is a wise restriction for general purposes, as I shall have occasion to emphasize, it might sometimes prove inconvenient, and would certainly put out of court a number of originals used in the first seven experiments, on which hits are found only in the experiments in which they were used.

On the other hand, if we were to attempt to assess hits on originals which did not appear in the Catalogue at all, taking  $p = c/N$  as above, we should find the value of  $p = 0$  (since  $c = 0$ , *ex hypothesi*) and this would give us an infinite score with indeterminate variance, which would be absurd. Accordingly, I have taken  $p = k/M$ , where  $k = h + c$  and  $M = n + N$ ,  $h$ ,  $n$ , and  $N$  having the same meanings as before; that is to say, I estimate the probability of a percipient drawing any object from data including the experiment in which he works as well as other relevant experiments.

This convention, while leaving us free to deal with any originals we please, regardless of whether the objects concerned are listed in any part of the Catalogue or not, imposes in general a handicap on the percipients inasmuch as, so to put it, we take their own performance into account in estimating the improbability of their achieving it. This handicap may sometimes be considerable; for example, if 2 percipients in 20 draw an object used as an original in their experiment, which, not used as an original before, was drawn by only one of the 741 percipients of the Catalogue, we shall obtain

<sup>1</sup> Since a percipient must clearly not be credited with more than one hit on any given original in any one experiment, this is in practice equivalent to saying ' $h$  is the number of hits made . . .'; on this understanding, the phrases will be used interchangeably as desired.



a score of 6.598 instead of 8.532. I have a further note on this point in section 17 below. For the experiment as a whole, the Score,  $F$ , is the sum of the scores for the different originals, and the Variance,  $V$ , the sum of the corresponding variances.

The method, with appropriate adjustments for particular purposes, has been used by Miss Jephson in experiments on card guessing (4), and by Besterman, Soal, and Jephson for the same purpose (5). It was put forward, in a suitably modified guise, by Saltmarsh and Soal (6) as the basis of a means of estimating the supernormal content of mediumistic communications, and was thus used by Pratt (7) in his study of Mrs. Garrett.

I should like to express here my great indebtedness to Professor Fisher for personally verifying the substantial correctness of my own application of the method to the present problem.

## 9

I think the easiest way of satisfying ourselves that the method is basically sound is by noting that, for any particular original, it is equivalent to the ordinary binomial treatment with the value of  $p$  given, for the factor  $\log 1/p$  cancels out, in the determination of Chi-square or of  $D/\sigma$ , leaving us with the familiar  $(h-np)^2/npq$  or  $(h-np)/\sqrt{npq}$  as the case may be. If all originals had an equal likelihood of being drawn by percipients under chance conditions the factor  $\log 1/p$  would be superfluous; and the whole method is perhaps best thought of as a binomial treatment in which the contributions made by hits on the various originals are weighted according to the improbability of the objects represented being drawn when no paracognitive factor is operative.

If it be asked why the weighting should be proportional to the *logarithms* of the inverse probabilities, and not according to any fancy function we might happen to choose, the answer is probably best given by considering the case of a percipient attempting what I might term a multiple guessing task in which the constituent independent elements are equally probable. Thus he might be required to guess cards bearing symbols of five different types (as Zener cards), each type being printed in five different colors, and any card bearing any number of symbols from 1 to 5. Given a pack containing equal numbers of all possible combinations of symbol, color, and number of symbols, his chance of getting any one attribute right, such as type of symbol, will be  $1/5$  of getting two right, such as symbol and color,  $1/25$ , and of getting all three right,  $1/125$ . But in order to get two attributes right, he has only to do twice as much paranormal cognizing as is needed to get one right—not five times as much; (i.e., he has to perform, in effect, two paracognitive acts instead of one)<sup>1</sup>. Similarly for the case of his getting three attributes

<sup>1</sup> The foregoing is on the assumption that the three attributes are cognized separately, in the way, for example, that would presumably occur with the values of three separate dice; this will by no means necessarily occur in practice, but whether it does so or not will not affect the illustration.

right, and it follows that scores which purport to give measures of the paracognitive ability displayed should be in the ratios of 1 to 2 to 3, not of 5 to 25 to 125. This desideratum can only be obtained by making the scores proportional to the logarithms of the inverse probabilities, since  $\log 5 : \log 5^2 : \log 5^3$  as 1:2:3. Extending this principle to the more general case, it is clear that where probabilities are in the ratio  $a$  to  $b$ , the scores must be in the ratio  $\log \frac{1}{a}$  to  $\log \frac{1}{b}$ . That is to say, the logarithm of the inverse probability is the proper weighting factor to adopt.

## 10

I suspect that the strictly logical thing to do at this stage would be to give as complete an account as possible of the Catalogue and of the principles and precautions to be observed in using it. But I think it will be preferable to proceed at once to the consideration of a concrete example of how the method is applied in practice, even at the cost of a slight derangement of logical sequence. There are, however, one or two points which must be noted before this can be done to advantage.

A glance at the Catalogue will show that the frequencies of occurrence of any object are given under three main headings, I-V, VI and VII. These refer to the experiments in which the drawings or mentions of the objects occur. I have pooled the data for the first five experiments, largely to save space, but partly because they form a natural group, both chronologically and operationally, partly because the total number of percipients, which was 250, agrees so closely with the 246 for Expt. VI and 245 for Expt. VII, and partly for a reason given at the end of the next section<sup>1</sup>.

Each of these three main headings is subdivided into two sub-classes, headed  $\alpha$  and  $\beta$ . It is important to realize, once and for all, that these categories have nothing to do with the merits of the drawing, or whether it is a 'good' hit on any original, real or imaginary; they are concerned *solely* with whether the object was the only one depicted in the drawing ( $\alpha$ ) or whether it was accompanied by others ( $\beta$ ). Thus a drawing of a horse, and nothing else, will be entered as an  $\alpha$  HORSE, but one of a horse in a cart, or a horse standing under a tree, will go down as a  $\beta$  HORSE and a  $\beta$  CART or TREE. Cases in which the percipient has made several distinct attempts on the same occasion have, of course, been treated as separate drawings. Decisions in borderline cases have been guided mainly by the common-sense likelihood of the objects concerned being found in such juxtaposition as to make them a plausible subject for a single composition.

<sup>1</sup> The number of percipients in Expts. I-V was given at 251 in my first paper. I have discarded one set which consisted of nothing but geometrical diagrams and indeterminate scrawls.

I have thought it worth while to preserve the distinction in the Catalogue, mainly for the benefit of any student who may care to investigate points connected therewith; but I have deemed it more prudent not to attempt to discriminate at present between the two categories, though there is some evidence to the effect that  $\alpha$  hits are superior to  $\beta$ ; I have accordingly used total hits ( $\alpha + \beta$ ) throughout, in all the calculations of this paper, unless otherwise expressly stated.

## 11

The other point that needs mention here is that of finding the 'net' values, so to call them, of  $c$  and  $N$ . In colloquial terms, the null hypothesis is to the effect that the probability of an object being drawn is unaffected by the fact of its being used as an original, and we must clearly test this only by comparing cases in which it is with others in which it is not. Thus, in determining  $c$  and  $N$  for any original, we must not include the number of drawings of the object concerned made in the course of any experiment in which it was used as an original, or the number of percipients making them; that is to say, we must subtract from the total frequency given by the Catalogue the number of hits scored in the experiment, if any, in which the object was used as an original, and from 741 the number of percipients engaged in that experiment. For example, the original house was used in Expt. VI, in the course of which 60 houses were drawn by the 246 percipients engaged. The total number of ordinary single houses drawn is given by the Catalogue as 153; so the value of  $c$  is  $153 - 60 = 93$ , and of  $N$  is  $741 - 246 = 495$ .

This procedure would be exactly correct if the influence of the original were confined strictly to the experiment in which it is used; but the discovery of the phenomenon of displacement (1. pp. 100 ff.)—which Dr. Thouless has also well named temporal dislocation of response—shows that this is not the case<sup>1</sup>. The influence of the original appears to extend, in a degree diminishing with remoteness, to experiments preceding and following it, so that the proportions of drawings of the relevant object which appear in them is liable to be somewhat higher than it would have been if the object had never been used as an original at all. The only way in which we could completely deal with this situation would be to determine the form of the relationship between probability of occurrence and remoteness from the occasion of use and then find from it the value to which the probability tends as remoteness is indefinitely increased. But this is quite impracticable, at any rate at the present stage, so we are

<sup>1</sup> The phenomena of displacement may be briefly summarized as follows: 'Paranormal cognition is not of a 'now or never' type. Genuine hits may occur either before or after the occasion of display of the original, though they are most likely to do so at or about the same time; that is to say, they may be 'displaced' or 'dis-located.' In fact, the probability of scoring a hit increases continuously as the occasion on which the drawing is made approaches that on which the original is displayed, reaches a maximum at or near the coincidence of the two, and falls off again as the former recedes from the latter.

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obliged to use the approximate method given above. The general effect of this will be to raise the calculated expectations above their proper values, and correspondingly to depress the scores, when originals used in Expts. I-VII are again used in fresh experiments, as they are likely to be from time to time on a random selection of originals. In so far as this occurs a handicap will be imposed on the percipients, which will provide a certain margin of safety.

The effect will be greater as the remoteness of the occasion concerned from that of the use of the original is less, and will accordingly be especially marked in the case of the first five experiments which were somewhat closely grouped (Experiments IV and V, for example, were separated by only five days), and I have accordingly thought it wise, as well as simpler, to treat Expts. I-V, from this point of view, as if they were a single experiment. Thus, in the case of any object used as an original in one of these experiments, we subtract from the total Catalogue frequency all the hits entered under the heading I-V, in order to obtain  $c$ , and take  $N$  as 491 in all cases; e.g., for HORSE, we take  $c$  as  $78 - 24 = 54$ .

I have applied the same policy, for the same reasons, to Expt. VII, of which the five sub-experiments were substantially concurrent and extended over no more than 23 days (2. p. 297).

There are other points which will have to be dealt with at a later stage; but the foregoing should enable us profitably to consider a concrete example in some detail.

12

For illustrative purposes I can do no better than select an experiment carried out by the Cambridge (Town) Society for Psychical Investigation, on June 5th, 1939, with their Honorary Secretary, my friend Mr. Edmunds Wing, acting as experimenter. This is particularly appropriate, as it happens to be the first independently conducted experiment of this type. It also possesses features which render it especially suitable for use in this way. In the first place, I regard it as more or less typical, both as to number of percipients (19) and results obtained, of the kind of experiment which the independent experimenter, working with a society or having access to a suitable class or group, may conveniently and profitably perform. In the second, it happens to provide good instances of the kind of minor problem with which experimenters are likely to be confronted in practice.

It also admirably illustrates the necessity and value of the methods here described. At the time, I could do no more than inspect the drawings, note with satisfaction that the percipients had apparently scored a good number of hits on the originals used, and put it in cold storage, so to say, pending the development of a practicable method of assessing it. The only alternative would have been to have had it cross-scored by a fresh judge against all or some of the first five experiments, which it immediately followed.

I should say here that it was a group experiment conducted under excellent operational conditions. The percipients were duly warned against discussion or 'cribbing' and were invigilated by the Chairman of the meeting; and the experimenter worked in another room, well removed from the one occupied by the percipients, contact being maintained by a messenger who could at no time see the originals. The originals were selected by inserting cards randomly in a dictionary and illustrating the first 'drawable' word so found. For various reasons I regard this plan as unsatisfactory for general use, but on grounds of convenience, not of validity (Cf. 2, p. 340), and it has worked well enough here.

## 13

The calculation, following the procedure of section 8 above, is given in full in Example I, p. 17. The arithmetic will be found, I imagine, self-explanatory and needs no comment here—except perhaps to remind inexperienced computers that the easiest way to find  $\log \frac{1}{p}$  is to subtract  $\log k$  from  $\log M$ .

On the other hand the determination of the values of  $h$  and  $c$  for the various originals raises several points of interest, which will serve as a good introduction to the general principles to be observed; so it will be worth while to consider the originals and the hits made on each, one by one, with some care.

The first, though nominally HERRING, I have treated simply as FISH. There is nothing very distinctive about a herring, or about Mr. Wing's illustration (as there might be about a shark, for example), and the only sub-class of FISH given in the Catalogue is Goldfish in Bowls. To my mind at any rate, goldfish-in-bowls are connotationally and conceptually very fairly distinct from fish in general; so I do not think it necessary to count those in the Catalogue in determining  $c$ . Moreover, of the 7 there given, 5 occur in Expt. VII, for which BOWL was an original, and it seems likely that they may have been determined by this.<sup>1</sup> We next note that of the 71 undifferentiated fish listed in the Catalogue, 21 occurred in one or another of the first five experiments, while FISH was an original for Expt. III; we accordingly subtract 21 from 71 to obtain  $c$  and 250 from 741 for  $N$ . Adding the three hits ( $h$ ) to  $c$  and the number of percipients (viz., 19) to  $N$  we obtain 53 and 510 as the values of  $k$  and  $M$ . Dividing the first of these by the second gives .10392 as the value of  $p$ , and the arithmetic then follows the obvious course as indicated by the column headings in the Example.

Only a single LEAF was drawn as the second original, but it would be unreasonable to reject sprays or the like of only a few leaves, so

<sup>1</sup> The effect of including them would be very slight; the score for the whole experiment falls only to 7.627 with variance 10.013;  $D/\sigma$  becomes 2.411 leaving  $P$  still less than .02.

I accept and count these. I also count from the Catalogue the two cases of 'plural, unspecified' Leaves, (no case among the drawings); but I feel this is rather unnecessary, though erring on the safe side.

There is nothing much to say about TURBAN, except that it is found only once among the drawings of I to VII and that there are no hits on it in this experiment. It is shown as on a rather vaguely indicated man's head; but I think it would be improper to count a man's head as a hit, as this is not the intention, so to say, of the original but merely incidental thereto.

## 14

The case of MARIGOLD deserves somewhat careful consideration. The original shows a 'ray and disc' type of flower, which might be a marigold but might just as well be anything else of the same general type. Among the drawings I find two quite definite 'ray and disc' type flowers, unnamed, a named tulip and rose, an unmistakable pansy, and "bunch of red flowers." I see no reason for supposing that any of these four last have anything to do with the original, whereas the two 'ray and disc' flowers are obviously as good hits as could be expected. Accepting these, we must ask what must be counted from the Catalogue to give us the appropriate value of *c*. Since the two ray and disc flowers of the drawings are unnamed, we must obviously start by taking all flowers in the Catalogue which are of this type, or approximately so. I accordingly take aster (2), chrysanthemum (8), cornflower (1), daisy (16), dandelion (2), marigold (9), marguerite (8), and sunflower (6). This gives a total of 52. But there are also 67 indeterminate flowers, which it would be improper to leave out of account. I think the only thing to do here is to assume (as seems reasonable) that if they could have been identified they would have been distributed in the same proportions as those that actually were. Now there are 233 classified flowers altogether, of which 52 are of relevant type, and 67 indeterminate; so I take  $67 \times 52/233$ , which is almost exactly 15, as the *pro rata* contribution of the indeterminates. Adding this to the 52 already found gives 67 as the best available estimate of the value of *c* in this case.

## 15

NAIL (Carpenter's) and SHELL (Conchological) call for no special comment, but BIRD presents a problem very similar to that raised by MARIGOLD. The bird drawn is of the 'ordinary or conventional' type, not flying, and the general principle of adhering as closely as possible to the original (of which I shall have much to say later) suggests that we should exclude flying birds, if any, on both sides of the account. The drawings include two unspecified birds and one "cuckoo." The original might as well represent this last as anything else—at least to the inexperienced eye—but it might just about as well be a crow (2), gull (5), pigeon or dove (4), possibly robin (1),



## EXAMPLE Ia

FISHER SCORES FOR 'WING' EXPERIMENT OF 5.vi.39.  $n = 19$ . STANDARD PROCEDURE.

No.	Original	$h$	$c$	$k$	$N$	$M$	$p$	$np$	$h-np$	$\log \frac{p}{1-p}$	Score	$q$	$npq$	$\log^2 \frac{1}{p}$	Vce
1	HERRING	3	50	53	491	510	.10392	1.9745	1.0255	.9833	1.008	.89608	1.7693	.967	1.711
2	LEAF	3	28	31	491	510	.06078	1.1549	1.8451	1.2162	2.244	.93922	1.0847	1.479	1.604
3	TURBAN	2	67	69	741	760	.00132	.0251	-.0251	2.8808	-.072	.99868	.0251	8.299	.208
4	MARIGOLD	2	6	6	741	760	.09079	1.7250	-.2750	1.0420	-.287	.90921	1.5684	1.086	1.703
5	NAIL	1	8	9	491	510	.01764	.3352	.6648	2.1026	-.315	.99211	1.487	4.421	.657
6	SHELL	1	34	37	491	510	.07255	1.3784	1.6216	1.7534	1.166	.98236	.3293	3.074	1.012
7	BIRD	3	3	3	741	760	.00395	.0750	.9250	1.1394	1.848	.92745	1.2784	1.298	1.659
8	KEYS	1	2	3	741	760	.01184	.2250	-.2250	2.4037	2.223	.99605	.0748	5.778	.432
9	STEEPLE	2	9	9	741	760	.01184	.2250	-.2250	1.9266	-.433	.99816	.2223	3.711	.825
10	MERMAID	2	1	1	741	760	.00132	.0251	-.0251	2.8808	-.072	.99868	.0251	8.299	.208
Total		13	206	319			.37200	7.0682	5.9318		7.884	9.62800			10.019

 $D/\sigma = 2.491; P = .02.$ 

## EXAMPLE Ib

ALTERNATIVE METHOD: 'MULTIPLE UNIT' PROCEDURE

No.	Original	$c$	$N$	$U$	$V$	$U'$	$V'$	$h$	$hU$	$kV$	$m$	$mU'$	$mV'$	Score	Vce
1	HERRING	50	491	.882	.0900	-.10091	.0900	3	2.646	.2700	16	-.1615	1.4400	1.031	1.710
2	LEAF	28	491	1.157	.0839	-.07084	.0832	3	3.471	.2517	16	-.133	1.3312	2.338	1.583
3	TURBAN	1	741	2.562	.0177	-.00387	.0111	2	1.886	.1794	19	-.074	.2109	-.074	.211
4	MARIGOLD	67	741	.943	.0897	-.09430	.0896	2	1.886	.1794	17	-.1603	1.5232	.283	1.703
5	NAIL	6	741	2.006	.0383	-.01692	.0351	2	1.886	.1794	19	-.321	.6669	-.321	.667
6	SHELL	34	491	1.706	.0542	-.02909	.0512	1	1.706	.0542	18	-.524	.9216	1.182	.976
7	BIRD	8	491	1.066	.0870	-.08020	.0866	3	3.198	.2610	16	-.1283	1.3856	1.915	1.647
8	KEYS	2	741	2.384	.0231	-.00692	.0177	1	2.384	.0231	18	-.125	.3186	2.259	.342
9	STEEPLE	9	741	1.845	.0465	-.02324	.0440	2	1.845	.0440	19	-.442	.8360	-.442	.836
10	MERMAID	1	741	2.562	.0177	-.00387	.0111	2	1.886	.1794	19	-.074	.2109	-.074	.211
Total		206				-.00387		13	15.291	1.0394	177	-.7194	8.8449	8.097	9.886

 $D/\sigma = 2.575; P = .01$

or rook (1), where the figures in brackets give the *net* Catalogue frequencies after subtracting from the totals the figures under I-V, because BIRD was an original for III. These give a total of 13 for classified birds, while the net value for unspecified birds, not flying, is 21, making a total of 34 as the best value of *c*.

For RAYS the experimenter drew an arc of a circle with a number of radiating lines, more or less as if it might be a rising or setting sun. I reject "sun, moon and stars," also "moon" among the drawings; but I feel obliged to accept "storm, with rain and sunshine between" with a drawing which definitely indicates sun's rays shining between clouds, and corresponds perfectly to the Rays of Sun given in the Catalogue.

The two remaining originals, STEEPLE and MERMAID, need no discussion beyond noting, in the case of the first, that it is neither necessary nor desirable to count churches, since the original depicts a steeple only; this, however, is a borderline case raising points which will be better considered at a later stage.

## 16

I hope that the foregoing short discussion will serve not only to introduce and illustrate the method, but also to strike what I should like to be thought of as the keynote of this whole paper, namely that of regarding both the subject as a whole, and such small practical problems of assessment or otherwise as may arise from time to time, in an entirely matter of fact and common sense way.

The essential situation throughout is this: The experimenter draws an X (i.e., any object), and a certain number of percipients also draw X's; we want to know whether this number is greater or less (and whether significantly so) than the number we should expect on the null hypothesis that it is determined by chance alone. We have no *a priori* probabilities to guide us, so we must make the best estimate we can from our empirical knowledge of what proportion of percipients does in fact draw an X when the experimenter does not. A considerable body of relevant experience is embodied in the Catalogue, and we accordingly use this as the basis of our estimate. As a rule this is perfectly straightforward, but it sometimes happens that we meet with some minor ambiguity, as with the originals of MARIGOLD and BIRD; in such cases we apply exactly the same kind of common sense methods that we should use in dealing with any other practical problem. A great deal of scientific work, and nearly all applications of science to the everyday problems of engineering and industry, depends on making just such estimates as these of quantities which are not given *a priori* but have to be determined from experience; and the fact that we are dealing with somewhat obscure instead of with familiar phenomena is no reason for not using ordinary straightforward methods to obtain the best estimates that the empirical data are capable of giving.



The reader will already have noticed that, in addition to the standard procedure for calculating scores and variances, Example I shows an alternative method of computation. This I must now discuss.

If we consider the case of a single percipient, in connection with any particular original, it is evident that the value of  $n$  in the standard formulae of section 8 above, will be 1, while that of  $h$  must be either 1 or 0, according to whether he scores a hit or not. If he does, his score is given by  $(1-p) \log \frac{1}{p}$  with variance

$pq \log^2 \frac{1}{p}$ ; if not, his score is  $-p \log \frac{1}{p}$  with the same expression for the variance as before. But the value of  $p$ , which is given by  $(h+c)/(n+N)$  is naturally not the same in the two cases, for it becomes  $(c+1)/(N+1)$  in the first case, and  $c/(N+1)$  in the second. Thus, denoting the score and variance appropriate to a hit in any case as  $U$  and  $V$ , and those appropriate to a miss as  $U'$  and  $V'$  ( $U'$  being always negative), it is easy to see that  $V'$  for any value of  $c$  such as  $r$ , is the same as  $V$  for the value of  $c=r-1$ .

For any given value of  $N$ , we may calculate once and for all, and tabulate, the values of  $U$  &  $V$ ,  $U'$  &  $V'$  (which may conveniently be referred to as Unit Scores and Variances) for all values of  $c$  likely to be met with or actually occurring in the Catalogue. This is done in Appendix II, for the four cases in which  $N$  is 741, 491, 496 and 495, corresponding to the cases respectively in which the object concerned has not been used as an original in any of the first seven experiments, or was used in one of Expts. I-V, or in Expt. VI, or in Expt. VII.

Then, if we regard the total score and variance appropriate to an experiment as equal to the sum of those appropriate to the participating percipients taken one by one, and if in the experiment  $h$  percipients score hits on an original (which is what  $h$  means) while  $m$  do not (so that  $h+m=n$ ), the score for any original will be  $hU-mU'$  with variance  $hV+mV'$ . Since this involves no more than multiplying tabulated values by usually small numbers, and adding or subtracting the products, the method is enormously less laborious than that of the standard procedure, and reduces the work required by, I should judge, nearly eighty percent. Thus, while a convenience at any time, it is almost imperative if the results of a considerable number of experiments, as in my Expt. VIII below, or equivalent variations of treatment, have to be computed.

To use the tables, first make sure that you are using the right value of  $N$ ; then, in the case of hits, take the score and variance under the headings  $U$  and  $V$  opposite the appropriate value of  $c$  found in the *left-hand* column; in the case of a miss, take the score (negative) and variance under the headings  $U'$  and  $V$  opposite

the same value of  $c$  found in the *right-hand* column. The application of these instructions and the subsequent treatment will, I think, be quite clear on reference to Example Ib. in conjunction with the tables.

It should be noted that, in order to save space, I have given in the tables only those values which are likely to occur in practice; special cases not listed, if any, can easily be worked from the formulae given above.

It will be noted that the two procedures do not lead to numerically identical results; as in the Example, so generally, the use of Unit Scores will give a slightly stronger result, whether positive or negative, than will the standard procedure applied to the same data. The question naturally arises as to whether this is pernicious, in the sense of so tending to produce spurious results as to unfit the more convenient method for general use.

The answer to this depends, I think, on the attitude we adopt towards the work as a whole and any experiment we may do in particular. If we regard each fresh experiment as a *de novo* test of the occurrence of paranormal cognition (or at least of a non-chance effect) then the standard procedure is the more correct. In this case we are, in the case of each original, virtually asking the question "Given that  $h+c$  drawings of this type have been made by  $n+N$  percipients, what is the probability that, as a result of chance alone,  $n$  percipients will draw  $h$  of them?" In this case, the pooling of the whole of the data from the experiment with those of the Catalogue is in accordance with standard practice. If on the other hand we accept evidence antecedent and external to the experiment as demonstrating, at least provisionally, the occurrence of some non-chance effect liable to recur in the experiment in question, then it is manifestly absurd to pool data, since this will only tend to obscure the effect we wish to study<sup>1</sup>.

In any event, the difference between the results given by the two methods will usually be small. Accordingly I have no hesitation in adopting and recommending the use of the much more convenient multiple unit method, as I may term it, for all ordinary purposes, with the reservation that the standard procedure should be used as

<sup>1</sup> If it be objected that, in these circumstances, we ought not to pool even one percipient at a time with the Catalogue data, the answer is that this is true, but that if we did not we should be rigidly confining ourselves to originals appearing in the Catalogue; otherwise we might find ourselves, trying to deal with an original of zero Catalogue frequency for which  $h$  was not 0, and this would lead to zero  $p$  with infinite score and indeterminate variance. Although, for various reasons, it is preferable as a rule to keep to objects appearing in the Catalogue when choosing originals, I see no sufficient reason for imposing this condition on anyone wishing to use the more convenient method of computation, and I have naturally been unable to observe it myself in cases where originals have been otherwise selected. I have accordingly thought it best to give the method and tables in the above form, even though it means obtaining slightly weaker results than are proper on the supposition that we are examining the data for the appearance of an effect assumed to occur elsewhere.

a check in doubtful or borderline cases. It has been used throughout this work except where otherwise expressly stated.

## 18

Two or three points remain to be cleared up before we proceed to the next Part of this paper, which deals more fully with the compilation of the Catalogue and the precautions to be observed in using it and in scoring hits in doubtful cases.

First of all it may be asked why we work by percipients, so to say, instead of by drawings. It might not unreasonably be suggested that what we want to know, in the case of any object  $X$ , is the empirical probability of an  $X$  being drawn under chance conditions, and that this will be given by the ratio of the number of  $X$ 's actually so drawn to the total number of drawings obtained under such conditions. The answer to this is as follows:

Assuming that the average number of drawings made by percipients working under chance conditions (i.e., when  $X$  is not used as an original) is equal to the average number made by those working in the experiment in which  $X$  is used as an original, the results of the two methods of estimating  $p$  will be very nearly the same. Take for instance the first line of Example Ia: here  $h$ ,  $c$  and  $k$  will remain as before, but since we are now working by drawings,  $N$  and  $n$  will be multiplied by, say, 10; the value of  $p$  will accordingly become .010392, but those of  $np$  and of  $h-np$  will remain as they are;  $\log \frac{1}{p}$  becomes 1.9833 giving a score of 2.034;  $q$  becomes .989608,  $npq$  is now 1.9540,  $\log^2 \frac{1}{p}$  is 3.9335 and the variance 7.686. This gives  $D/\sigma = .7337$  and  $P = .463$  (for this original only, of course, not for the whole experiment) compared with .7706 and  $P = .449$  from the score and variance of the Example, and the reason for the discrepancy is the answer to the question raised. In any problem of this type we are concerned with finding a *cumulative* probability, that is to say, the probability of obtaining by chance alone a result as great as that observed, or greater; in this case the probability of obtaining three or more hits on the original HERRING when the 491 relevant percipients of the Catalogue score 50 hits between them. Working by percipients we envisage, so to say, a possible maximum of 19 hits, i.e., the possibility that every percipient will draw a Fish, as he would if paranormal cognition were 100% effective. But if we were to work by drawings we should be tacitly envisaging the possibility of a maximum of 190 hits, i.e., of every percipient drawing 10 Fish—which is absurd—and the treatment, like any other binomial method, makes due allowance for this. We are not, of course, interested in a percipient drawing a plurality of  $X$ 's, because, apart from statistical difficulties, we have no kind of assurance that they would represent

independent cognitions. Thus it is the proportion of percipients drawing X, not the proportion of drawings of X in all drawings, that is the proper basis for assessment.

Closely connected with the foregoing is the question of what allowance, if any, should be made for the facts (1) that the average number of objects per percipient drawn by the percipients of any particular experiment will not in general be exactly equal to the average number per percipient drawn by those of the Catalogue, (2) that individual percipients draw differing numbers of objects. Theoretically, of course, such points should not arise; in an ideal experiment each percipient would draw one simple unambiguous object only at each trial, making ten in all, but this ideal is quite unrealizable in practice and from the start I regarded it as undesirable to attempt to force percipients into a rigid procedure which might just serve to inhibit the effects I was looking for, and experience indicates that I should not have succeeded if I had tried to do so.

Other things being equal, the more objects a percipient draws, the greater will be his chance of drawing the right one. Thus, if in some fresh experiment the percipients were to draw on the average twice as many objects as were drawn by the Catalogue percipients, their expectation of hits would be approximately doubled, and the method used, based on the assumption that the average number of objects drawn is the same in the two cases, would be misleading. Conversely, if they draw many fewer than did the Catalogue percipients, the calculated expectations would be too high and the experiment correspondingly handicapped.

We may make allowance for this, if we wish, by taking as the 'equivalent' number of percipients in the experiment,  $n'$  say, that number which would have produced the same number of objects as the percipients actually did produce if they had been producing at the same average rate as the percipients of the Catalogue. That is to say, if the  $n$  percipients of the experiment produce a total of  $r$  objects between them, and those of the Catalogue produce  $R$ , we may take  $n'$  as  $741r/R$  to the nearest whole number—or, for caution's sake, the whole number next above any fractional value actually obtained.

In complete strictness, I suppose, this adjustment ought always to be made, but in practice it is very seldom necessary. The reason is that the latitude allowed the Catalogue percipients in the instructions given them (Cf. section 23 below) led to their drawing a considerably larger number of objects, on the average, notably in the form of 'scenes' and other composite drawings, than the percipients under the more modern instructions which I have found preferable and discuss later. Consequently, it is sufficient, so far as safety is concerned, to satisfy oneself that the percipients of an experiment have not averaged appreciably more than the Catalogue percipients.

It will be seen on reference to the Summary of the Catalogue that, even when we have eliminated all geometrical diagrams, indeterminate scrawls, and unassessable, illegitimate or doubtfully legitimate objects (Cf. section 25) we are still left with a total of nearly 8,500 objects for the 741 percipients—an average of about 11.4—whereas it is unusual for percipients under modern instructions to average so many as 10. I have accordingly made it a practice to take  $n$  at its face value, except in the very rare cases where the percipients have averaged higher than those of the Catalogue<sup>1</sup>. It is true that this imposes a small handicap on the experiment, but this is on the safe side and so trifling as to be negligible in any ordinary case.

It is, perhaps, just worth noting here that the use of a value of  $n'$  greater than  $n$  will introduce a small error in the direction of safety of the kind discussed in the preceding section, for the method proceeds on the tacit assumption that a maximum of  $n'$  hits can be scored on any original, whereas only  $n$  can be.

## 20

As regards the second point, I think it is clear on general grounds that, once we have allowed as above for differences in the average number of objects drawn by the percipients of the Catalogue and those of any experiment, there remains no more than a kind of second order effect operative only in so far as the proportions of percipients giving themselves relatively greater (or smaller) chances of drawing any particular object, by drawing numbers of objects greater (or less), to any given extent, than the mean, differs as between Catalogue and experiment; that is to say, in so far as there is a difference in the relative standard deviations of the frequency distributions of the numbers of objects drawn in the two cases. Since by inspection these manifestly differ very little, the effect may safely be neglected. Moreover, inasmuch as the tendency, such as it is, appears to be in the sense of an excess of relatively large numbers in the Catalogue, as compared with experiments conducted under modern instructions—which would have the effect of tending to raise the expectation for any experiment under examination—the effect is likely to operate, in general, in the direction of safety.

## 21

Finally, there is the question of the skewness of the distribution of the scores. Any binomial treatment involving values of  $p$  not equal to .5 is bound to give a skewed distribution, and since the values of  $p$  in this case may be very small, the skewness may be severe, and in such cases the application of a standard error to a single value of  $h-np$  might give highly misleading results. For example, if a single percipient scores a hit on an original for which  $c$  is 4 and  $N$  is 496, the method will give a score of 1.977 with variance .0397; this would lead to a value for  $D/\sigma$  of about 10, with

<sup>1</sup> No such case has been observed among any of the experiments discussed in this paper.

an 'astronomical'  $P$ , of which the proper value is evidently only about .01.

Fortunately, however, the distribution of the *sum* of a number of quantities such as these scores tends rapidly to normality as the number of quantities summed is increased, so that we shall seldom be misled when, as in this work, we are dealing with the sum of ten scores or even somewhat fewer<sup>1</sup>. I am much indebted to Professor Fisher for having reassured me on this point, after examining some specimen calculations including those of Example I and the most extreme case I have yet encountered.

As a matter of fact, there is no reason why we should ever be misled at all, provided we always bear in mind what scores and tests of significance are for. In so far as the scores we calculate are used for what I may term 'informational' purposes, such as enquiring whether women are better percipients than men, older persons than younger, etc., or whether originals of animals, say, are more effective than those of domestic utensils, we shall presumably be summing fairly large numbers of scores, in which case there is no difficulty. But if we are testing significance in a single experiment we are, or should be, interested solely in the question of whether the facts observed are likely to be due to chance, and it is (or should be) a matter of indifference to us whether we obtain a value of  $P$  of one in a hundred or one in a hundred million; for each assures us, and the second little more strongly than the first, that chance is unlikely to be responsible, and this is all we want to know. It is only in borderline cases where the value of  $P$  is close to the conventional limit of one in twenty that we need feel any anxiety, and even here the method will only be misleading (in a formal sense) when the number of effective originals is small<sup>2</sup> and the percipients happen to have pulled off a long shot. But such cases are rare in properly conducted experiments, and are readily identified by inspection for treatment with the appropriate caution. This amounts to little more than reiterating the obvious principle that it is injudicious to form any but the most tentative opinions on a very small number of observations.

<sup>1</sup> There is, of course, no theoretical necessity for using ten originals, rather than eight or twelve or twenty; but it is a convenient number, and as the work has been conducted on this basis hitherto, it may as well be standardized for general use.

<sup>2</sup> I say 'effective originals' because, if the experimenter uses originals which are not in the Catalogue, and no percipient scores on them, they will contribute nothing to the calculation and are as if they had not been used.



## Part II

### Description of the Catalogue: Principles of Scoring Hits

#### 22

I must now give some account of the Catalogue presented in Appendix I, but before going into details there are some points of general interest and importance to be considered.

Any method of assessment which depends on the comparison of one set of observations with another involves the assumption that the two sets are similar in all relevant respects except that in which we are interested, and if this assumption is false the method is invalid. In this case it is assumed that the percipients of any experiment, and those whose drawings are summarized in the Catalogue, are substantially random samples of substantially the same population; or at least that any differences there may be are not relevant to the issue.

The 741 percipients of the Catalogue were all persons drawn from the higher educational classes of what I may reasonably term Western European culture (with which I include the United States of America)—as opposed to, say, Indian, Negro or Chinese. On a rather rough estimate, about 80% of them were students of between 18 and 23 or 24 years of age, and there was a slight preponderance of women (about 55%). The great majority were British, but there were 82 Dutch in Expts. IV and V, 12 from U. S. A. in Expt. IV, and about the same number in Expt. VI.

Ideally, of course, we should prefer any experiment destined to be assessed by means of the Catalogue to consist of precisely the same proportions of these various classes; but this will seldom if ever be realized in practice, and we must briefly consider what effects are likely to be produced by such differences as may obtain.

On the most general grounds, it would be injudicious to use the Catalogue, except as a matter of interest, for assessing experiments performed on percipients of a cultural background differing very widely from that indicated above, on mental defectives, persons of conspicuously low educational attainments, or very young children. Apart from such extreme cases, however, it seems fairly clear that the effects due to differences between the percipients of an experiment and those of the Catalogue are likely to be small, and moreover—which is important—that they will tend in general in the direction of safety.

In the first place, if the population from which the sample of percipients in any experiment is taken is liable to draw a higher percentage of one type of object than that of the Catalogue, then it must be liable to draw lower percentages of some other type, since the sum of the percentages must be 100 in each case. Thus, for a

plurality of randomly selected originals, the two effects will tend to cancel each other, on the average. In any particular case, of course, there is liable to be some residual effect, which might be appreciably misleading (in one direction or the other) if there were a strong tendency for different groups to favor different objects or types of object; but scrutiny of the material fails to indicate, in fact, any considerable tendency of the kind, apart from the effects produced by the use of the originals.

In the second place, it is easy to see that the effect of a difference in population is much more likely to lead to an overestimate of the expected number of hits than to an underestimate—at least on the eminently plausible assumption, which the facts confirm, that people are more likely, under chance conditions, to draw familiar objects than unfamiliar, and *provided* the originals are selected only from among objects appearing in the Catalogue. If, for example, our Catalogue had been compiled from drawings of a population of 14th century Englishmen, we might reasonably expect that it would contain a higher proportion of bows and arrows than does the present specimen, but no motor cars, radios, or telephones. If, then, in a present-day experiment, we were to use BOW as an original, the expected number of hits, calculated from such a Catalogue would be too high, and the score correspondingly too low. Of course, if we were to use MOTOR CAR or TELEPHONE, we should get a grossly exaggerated score, since these objects, *ex hypothesi*, do not appear in the supposed Catalogue, but under the proviso made above this very circumstance prevents us from using them. This imaginary example illustrates one reason for adopting the principle that objects not listed in the Catalogue should not be used as originals, or at least only with the greatest discretion.

No doubt exceptional cases could be found, and may possibly occur in practice on rare occasions; but I think the foregoing should suffice to show that the risk of obtaining spurious positive results due to differences between experiment and Catalogue populations is one which, in all ordinary circumstances, may safely be neglected.

## 23

A more important question, from the practical point of view, is that of differences in the instructions under which the percipients work. Evidently if the percipients of the Catalogue had been discouraged from drawing any particular type of object, while those of some later experiment were not, the calculated expectations for such objects would be too low, and a spuriously high result would be obtained, if originals of that type were used in the latter experiment. If, for example, the percipients of the Catalogue had been told not to draw human beings, the Catalogue would contain entries under HUMANS derived only from those who had ignored the instruction, and if the prohibition were not imposed in some further experiment, while an original depicting a FISHERMAN were used, the percipients



would be likely to draw more representations of these patient optimists than a study of the Catalogue would lead us to expect, even in the absence of paranormal cognition, and there would be a risk of obtaining a misleadingly high score.

Fortunately, however, the reverse is the case, for I began by imposing virtually no restrictions on my percipients, but gradually tightened up the instructions, in the hope of eliminating useless material.

In Expts. I, III, IV, and V, the printed instructions merely said "Certain drawings, all of which will be quite simple, will be displayed . . . etc. . . . You are asked to reproduce these as well as you can." Equivalent instructions were given verbally in the case of Expt. II.

In Expt. VI, I said "Do not draw vague scenes, elaborate interiors or geometrical diagrams," and repeated this for Expt. VII with the minor qualification "unless your impressions are particularly vivid." In this last case I further suggested that percipients should "reject images of which you clearly recognize the origin," in the hope of getting rid of some of the too facile local associations or suggestions; but I think the only result was somewhat to harass percipients, and I fail to see how it can have had any appreciable selective effect. Apart from this recommendation, the Instructions for Expt. VIII, the results of which are discussed below, in Part III, B, were substantially the same as for VI and VII.

It will be seen from the Catalogue that the effect of these changes in instructions was virtually to eliminate geometrical diagrams from VI and VII, and these are excluded altogether from the body of the Catalogue. The effect on 'scenic' items, on the other hand, seems to have been negligible, as judged by reference to such entries as RIVER, LAKE, SEA, FIELD, MOUNTAIN, etc., etc. But in any event these are not recommended for use as originals, for reasons which will be given in Section 25 below. We may accordingly feel confident that there have been no changes in instructions liable to lead to spurious results.

It is, however, highly desirable that the experimenter should impress on his percipients the fact that the original will always depict some single and simple concrete object (such things as ANGEL, DEVIL and GNOME being 'concrete' for the present purpose), and will not be a diagram or a scene. Otherwise they may unnecessarily reduce their chances of scoring hits by drawing objects or mentioning items which could not possibly have been used as originals. Experience shows that it is almost impossible to ensure unanimously intelligent behavior in this respect, no matter how carefully or emphatically instructions are framed; there seems always to be a few percipients who insist on putting down such things as 'A smell of turpentine,' 'Fortitude,' 'A feeling of nausea,' or 'The mentality of Professor Joad,' but again such aberrations are methodologically harmless and serve only to impose a slight additional handicap on the experiment.

We may now turn to a consideration of the arrangement and contents of the Catalogue itself.

I need hardly say that, in presenting it for the use of future experimenters, I am inevitably more conscious of its defects than of its merits. One is naturally apt to forget the literal thousands of entries which have dropped neatly and unambiguously into their proper place, and to remember more vividly the few score instances in which difficulty has been encountered. Still, although I should be sorry to claim for it even an approximate perfection, I believe that it presents as accurate and exhaustive a picture of the output of the 741 percipients considered as could reasonably be expected; and I should be much surprised, as well as deeply chagrined, if an omniscient auditor were to tell me that it is likely appreciably to mislead an experimenter so often as once in a hundred experiments.

As regards the general arrangement, I have already explained, in section 10 above, my reasons for grouping together the percipients of the first five experiments; and also the difference between  $\alpha$  and  $\beta$  entries, namely, that the former refer to cases in which the object concerned was the sole constituent of the drawing, and the latter to those in which it was accompanied by others; but the term 'virtually sole' needs some amplification at this point. By it I mean that I have ignored various minor items, not uncommonly met with, which have manifestly been inserted as incidental trimmings or the like and have no claim to be considered as an essential part of what the percipient wishes to proffer as his attempt at scoring a hit. For example, many boats are adorned with a small flag, scenes often include conventionally indicated birds, and so forth. It seems to be absurd to suppose that such ancillary trivialities are to be regarded as serious attempts to identify the original, and they have accordingly not been listed; but inasmuch as it is difficult to draw the line in certain cases, or to set a limit to the extent to which a composite drawing may be built up round an item as nucleus which may appear of very secondary importance when it is finished, I have tended always towards inclusion rather than exclusion. I have throughout borne in mind the ruling principle that the one fault the Catalogue must not on any account possess is that of tending to cause the experimenter to underestimate the expected chance performance of his percipients, and I have therefore consistently overcatalogued rather than undercatalogued, so to say, in cases of doubt; but I do not suppose that these amounted to so many as 1% of the whole.

The reader will notice that the cross-headings of the Catalogue, i.e., the names of the various objects, are set in three sorts of type, namely SMALL ROMAN, ordinary, and italic. I use the first of these for objects which I regard as entirely suitable for use as originals, or substantially so, and of which the frequencies entered are correspondingly suitable for use in assessment; the ordinary type is

used for subheadings of these, or for descriptive matter, etc.; the italic indicates that I regard the objects, etc., concerned, or varieties thereof, as *unsuitable* for use as originals, though only those of the type discussed immediately below should be regarded as definitely illegitimate.

The reasons for this distinction are as follows: I have thought it proper to give *all* the objects drawn or mentioned, subject to the reservations made in the preceding section or below, insofar as is humanly practicable; otherwise it might be suspected that some process of wish-determined selection had been used tending to make the use of the Catalogue more 'profitable,' so to say, than it should be. But this policy of completeness, if unthinkingly pursued, might easily lead to absurd and deceptive results, as a simple illustration will show.

Suppose a percipient draws and mentions 'rolling clouds': clearly the above policy requires that I enter this under *CLOUD*, and rightly so, for there is unimpeachable evidence that the idea of 'clouds' was prominently present in his mind. Another percipient draws a horse under a tree, and vaguely indicates a suggestion of clouds in the sky. Are these also to be entered under *CLOUD*, despite the fact that what the percipient wished to present was obviously a horse, or perhaps a tree, and that he probably was not thinking of a cloud in any serious degree at all? Again, a percipient will occasionally specifically mention the sea, or a field, in a manner which the policy of completeness forbids us to ignore; but almost every boat or ship is depicted more or less overtly on a sea (or possibly a river, etc.) of sorts, and every horse, cow or sheep in a field. In view of the fact that the percipients of these experiments were not instructed (as I now strongly recommend) to state explicitly what their drawing was intended to represent, it is not possible to draw the line between the cases in which sea, field, etc., was the primary and intended object, and those in which it was secondary or purely incidental. These objects are accordingly unassessable, and therefore barred from use as originals. Similarly, since the frequencies of their relevant occurrence are not known with precision, these must not be involved in any process of assessment based on the Catalogue.

I have not distinguished these definitely 'illegitimate' items by the use of a separate sort of type, and the experimenter must use his own judgment and common sense in identifying them if occasion arises—which will be easy enough if the above principles be borne in mind. But a careful count, extended for safety to cover a considerable number of doubtful cases, gives the figures shown as 'Illegitimate or Doubtful' in the Summary of the Catalogue; and it is by subtracting these from the totals for *ROMAN* and *italics* combined that the figures for 'Net Relevant' Items are obtained.

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I have also relegated to the italicized class a considerable number of miscellaneous entries which I consider to be *unsuitable* for use as originals on various grounds other than those just discussed.

The second of these, *alidade*, is a good example. I doubt whether one person in a hundred knows what an alidade is, or what one looks like, or would recognize one if he saw it. It would accordingly be absurd to use it as an original, except perhaps on the completely unwarrantable assumption that, if paranormal cognition occurs at all, it must be of such a type that the percipient is able accurately to copy a drawing of which he does not recognize the content. The whole technique—or, indeed, any other of empirical type—is based on the assumption that an object has as good a chance of being drawn, so to put it, by the percipients of any experiment as by those with whom they are compared, and this would be falsified if originals were used depicting objects known or familiar only to a restricted class of percipients of whom a few happened to be included in the Catalogue. To do this would risk improperly handicapping the percipients to no good purpose; but we could only improperly aid them by inserting in the Catalogue, or selecting for use as originals, objects which the Catalogue percipients have not drawn, but which it is believed that future percipients are likely to draw; in this respect the position here is analogous to that discussed in section 22 above.

On the other hand, the method also assumes that the percipients of any fresh experiment are given, or give themselves, no better chance of drawing any particular object than those of the Catalogue had. But this would be falsified if the latter had been allowed or encouraged, by differing instructions or otherwise, to draw so large a proportion of a type of object which the former are not allowed to draw as to make their average relevant and comparable output lower than that of percipients working under altered instructions. But this is not the case, as pointed out in section 19, and in any event could be dealt with by the adjustment therein described. As it happens, the average number of items produced per Catalogue percipient is somewhat higher than that to be expected in fresh experiments, so that the straightforward use of unadjusted Catalogue frequencies gives a margin of safety in all ordinary cases; but if an adjustment has to be made, owing to an abnormally high (or low) output by the percipients of any particular experiment, the total for the Net Relevant entries (8,463) should be used, as the value of R in section 19, not that for all entries.

I have accordingly italicized a considerable number of unduly rare, technical, obscure, recondite, vague or ambiguous objects, which experience and common sense suggest are unsuitable for use as originals. It is not necessary that anyone should agree with me in all these cases, or everyone in any of them and no doubt some of them will appear arbitrary; but I think the reason for most of them will be clear enough on reflection. There is, however, no methodological reason against any experimenter's using them for special enquiries if he sees fit to do so, except, of course, for the definitely illegitimate items discussed in the preceding section.

It must be remembered that although italicized items may be unsuitable (in my judgment at least) for *use as originals*, on one ground

or another, they are none the less, in most cases, perfectly legitimate objects for percipients to have *drawn* (or mentioned) if they happened in fact to come to mind; the only exceptions are those (notably of a more or less 'scenic' type) which percipients could not have drawn if they had adhered to the instructions given in the later experiments (Cf. section 23 above). All other items, however 'unsuitable' on grounds of rarity, oddness, etc., must be retained in any estimate of the average output of percipients, etc., such as that used in section 19.

It should be noted, however, that a modicum of common sense may occasionally be needed in dealing with these two types of entry. For example, I list *ARROW*, Full and *ARROW*, *Partial*, and italicize the second of these. This is because I have necessarily counted conventional indications of the 'barb and shaft' type (i.e., → or approximately so) as hits on *ARROW*, and felt it would add to the interest and completeness to show the frequencies separately. Now, it would clearly be foolish of the experimenter, if he decides to use *ARROW* as an original, to draw only a part of the object, and that is why I have italicized the *Partial* entry; but in assessing hits we must certainly count these frequencies, since most of them were evidently intended to represent arrows. But cases such as this are very rare, and I think the individual experimenter will have no difficulty in identifying and dealing with them.

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Two further points may be noted before I go on to describe the rules which governed the compilation of the Catalogue.

First: Many of the subheadings are given as a matter of interest and for the possible convenience of future experimenters rather than for any methodological significance. For example, I list open and closed *UMBRELLAS* separately, because an experimenter who has satisfied himself that an effect of some kind occurs might well wish to enquire into such points as whether the use of an original depicting an open umbrella tended to elicit a higher proportion of drawings showing the object open, as compared with closed, than occur under chance conditions. The same applies to cups with and without saucers, to open and closed boxes, and the like. But I think we should naturally and rightly accept any umbrella (open or closed) as a hit on an original *UMBRELLA* (closed or open) in the absence of knowledge on the point, though it would be legitimate, if rash, to 'declare to win,' as racing parlance has it, with either one *before* seeing the drawings; and we certainly must do so (or unfairly handicap the percipients) if any percipient merely mentions 'an umbrella' without drawing it and without specifying its condition. Naturally, we must then count both kinds in determining *c*.

In general, therefore, the frequencies of subheadings should be pooled in assessing results, though not, of course, if they refer to markedly different objects, such as *BOW* (archery) and *BOW* (ribbon) or *SHIP* (sailing) and *SHIP* (steam).

Second: Probably the best way, though somewhat laborious, of tackling the assessment of an actual experiment is to have a list made—preferably by someone who does not know what originals have been used—of all objects drawn or mentioned by the percipients, showing the frequency of occurrence of each and also its Catalogue frequency. It can then be decided—again preferably by persons not knowing the originals—on the principles discussed in II.B. below, and with such consultation, etc., as may be necessary, which of these objects are to be deemed hits on the originals used. This will not solve the difficulty of deciding what are to count as hits in doubtful cases, but it has the merit of getting the initial classification objectively carried out before these questions arise. Moreover, such lists will naturally form the nucleus of a 'local' catalogue, and there can be little doubt that compilation of a catalogue is calculated to give the experimenter an understanding of the kind of things that people are in practice likely to draw, and an insight into the kind of lines on which their minds work in choosing them, which it would be difficult to acquire in any other way, and is likely to be helpful both in forming decisions in borderline cases and in detecting apparent anomalies which may be worth following up; in other words, it is the best way of ensuring that practical first-hand experience of the subject which no set of rules and formulae can ever fully replace.

## 28

Apart from the exceptions noted above, from such others as may be given below, and from a few negligibly rare cases in which particular local circumstances warrant and demand special treatment, the following rules and principles have been observed throughout the compilation of the Catalogue:

1. Every object drawn or mentioned has been entered; there has been no omission of objects just because I thought (as often enough) that they were stupid things to draw, or because they were unsuitable for use as originals. In particular, everything specifically mentioned has been listed, even if not drawn or drawn only trivially. But mentions of mental states and feelings, abstract qualities, etc., colors and materials are not listed. Indeterminate scrawls, etc., and geometrical diagrams, patterns, etc., have been counted but not classified; they are of course italicized, and will be found in the Summary.

2. Nothing is listed twice, except in so far as it may have been necessary to enter two objects of the same 'genus,' so to say, but of different 'species' under two subheadings. Thus if a percipient draws two trees of the same type in a single set of drawings, only one would be entered; but if he were to draw one palm tree and one fir tree, we should have one entry under *TREE, Palm*, and another under *TREE, Conifer*.

3. In general, a plurality of objects of the same kind is treated as a single object of that kind; e.g., a drawing of three cats is treated as if it were a drawing of a single cat, and gives us only one entry



under CAT. Such pluralities are only given a separate heading in special cases, as where it might be convenient to distinguish the plural from the singular (e.g., single sheep and flock of sheep) or where the plurality is inadmissible or doubtfully admissible as a hit on an original depicting the single object (e.g., mention of a street as a hit on an isolated HOUSE).

4. In general, where common sense requires it, parts of wholes have been taken as equivalent to the wholes of which they are parts; e.g., a horse's head is entered as a hit on HORSE, the corner of a table as one TABLE, the bows only of a ship as one SHIP, etc. But a drawing of a nose, or of an eye, ear, hand, foot, etc., is not entered under HUMAN BEINGS, but under NOSE, EYE, etc.; similarly, drawings of windows, chimneys, doors, spires, etc., drawn in isolation give no entries under HOUSE or CHURCH but only under their own headings.

Conversely, drawings of men and women, who may be presumed to possess noses, etc., or of houses, to which windows, chimneys and doors are accessories that may be taken for granted, give no entries under these headings for the associated or component parts, but only under those for the objects as wholes.

In short, parts of the body, or accessory parts of wholes, occurring in natural conjunction with the wholes of which they are parts, are not separately listed unless specially mentioned; and not even then in the case of parts of the body. The chief reason for this last convention is that many percipients of the early experiments indulged in more or less elaborate descriptions of imaginary persons, usually such as they conceived to be associated with the experiment, and it is not practicable to draw the line between such effusions and genuine impressions directed at the identification of the original.

5. Similar principles have been applied to garments, etc., and these—in particular, hats, coats, trousers, dresses, boots, shoes—drawn or indicated as part of the normal clothing of human figures, are not separately listed unless specifically mentioned, except in two or three rare instances where the garment, etc., has been drawn with such emphasis as compared with the rest of the figure, etc., as to be tantamount to special mention.

6. I have never paid any attention to the *shape* of the drawing as such, except in that it is necessarily by the shape that one decides what the drawing is intended to represent—unless, of course, the percipient happens to have written this down. In one or two cases, it is true (e.g., COMET, GRINDSTONE), I have made entries even though I doubted whether the drawings were really intended to represent the objects in question; but in each such case the drawing irresistibly suggested the object immediately on inspection, and resembled it so closely that it would have been unreasonable to refuse to concede it as a hit if the object had been used as an original. I have never entered an indeterminate scrawl as an object because to an imaginative eye it 'looked rather like' or 'might be meant for' something or other. The procedure has always been to inspect the

drawing, decide what it represents in the plain common sense meaning of the term, and enter those objects and those only. Fanciful interpretations of the 'puzzle picture' type have been absolutely and rigorously barred, and if I could not easily decide what was represented, the drawing was entered as Indeterminate, e.g., if the drawing might equally well have been a stone or a potato, a chopper or flag.

7. If, on the other hand, it is the percipient who is in doubt as to the nature of his impression, so that he says "An X or a Y," I have made an entry under both headings, because it is clear that he must seriously have entertained both ideas. Occasionally it happens that a percipient draws one thing and describes it as another, e.g., a drawing of a gimlet is described as a corkscrew; here again, and for approximately the same reason, an entry is made under each heading.

8. Mention of "a picture of an X" or "a model of an X" count as if an X had been drawn.

9. Composite drawings, as already implied, have been 'dissected' into their component parts. Thus a drawing of a bowl of fruit, in which apples, bananas and grapes are distinguishable, will give entries (all  $\beta$  of course) under BOWL, APPLE, BANANA and GRAPES; but if the percipient were to write down "A Bowl of Fruit" without drawing anything, or if the kinds of fruit drawn were indistinguishable, we should have entries under BOWL and FRUIT, Indt. only.

10. In cases where there are two or more subheadings for an object, none of them being 'Unspecified,' and a percipient mentions an object of that general sort without specifying the particular variety he has in mind, the entry is made under the commonest heading. For example, a mention of "A lamp" would be entered under LAMP, Table, various. This convention can clearly have no effect on cases in which the frequencies of the subheadings are pooled, but will introduce a slight bias in favor of the commonest type of object if there be a question of comparing types, e.g., the open and closed umbrellas mentioned above; but such cases are very rare and may, I think, safely be neglected for all ordinary purposes.

11. In the absence of positive identification by the percipient, which I think should henceforward be insisted on so far as possible, it fairly often happens that, although there is little doubt as to the general character of the object drawn, e.g., a helmet, it is difficult to be sure just what variety is intended. In such circumstances I have often used the sign  $\sim$ , meaning 'approximately,' 'substantially,' 'as nearly as can be judged,' or words to that effect, or the corresponding adverbial forms where the context requires. The use of this is not to be taken as implying that the object is indeterminate, but only that its precise nomenclature is in some doubt. I have also occasionally used it in other connections, of which the nature should be clear from the contexts.

The foregoing does not purport to be an exhaustive account of how every doubtful point has been settled; but I think it covers the



principal conventions adopted, at least sufficiently to enable an experimenter to understand the Catalogue and use it intelligently, and to compile a substantially comparable one of his own if he so desires.

I should like to emphasize again, however, that the proportion of cases requiring the application of these rules and conventions is small, so far as most of them are concerned, compared with the great majority in connection with which no question presents itself.

29

The reader will notice that I have grouped all varieties of birds, flowers and human beings together under appropriate subheadings in the three cases. I have not done this for various other categories such as animals, insects, fruits, etc., because it seems to me that, whereas in certain circumstances it might be desirable, or at least permissible, to pool all entries of birds, or of flowers, or of men or women, one could hardly wish to do so for the other categories. Percipients are, in actual fact, very likely to draw vague, unspecified flowers, birds, men and women, whereas indeterminate animals, insects, etc., are rare; and this confirms my own impression that from the relevantly psychological point of view, animals, etc., are much more sharply differentiated than are birds, flowers, etc.

In this connection, I think I should say that the classification of flowers is by no means perfectly reliable. In cases where the percipient has named the flower drawn, there has, of course, been no difficulty; and a few, such as tulips or daffodils, have usually been easily identified. All doubtful cases were reviewed by a comparatively expert horticulturist, who was able to identify a considerable number of them with reasonable assurance—at least in the sense of saying what the drawing appeared to represent; these were entered under the appropriate headings, and the remainder relegated to the "Indeterminate or Unspecified" class. But it is quite possible that in some instances the percipient had no more than 'a flower' in mind and only happened to draw something which looked like a particular flower.

In the cases of birds and human beings, however, all particular varieties were either explicitly named or virtually unmistakable.

30

Before concluding this necessarily somewhat fragmentary account of the Catalogue, there are two further points:

First: Although the Catalogue has been compiled with the greatest possible objectivity, and attains, I believe, a high degree of accuracy, I do not regard its arrangement, at any rate in detail, as more than tentative and experimental. I should accordingly be glad to receive suggestions for the improvement of future versions (if any), especially such as may be prompted by actual experience of its use.

Second: The Catalogue is primarily intended to enable future experimenters to assess the results of their experiments; but it seems worth while to point out that there is a certain amount of exploratory work which the interested reader might carry out with the aid of no more than the data presented in it. For example, we might set out to enquire whether objects related in some defined way to those used as originals—as by being commonly associated with them, or by assonance of their names, or otherwise—tend to appear relatively more often in experiments in which the related originals are used than in others. The question of what objects are 'commonly associated,' etc., should preferably be determined by reference to some external work on the subject, and some care would have to be exercised, notably in avoiding pairs of objects so related as to be very likely to appear in a single drawing (e.g., Blackboard and Easel, Locomotive and Train); but the results might be very interesting.

I mention this possibility, without elaboration, purely as a passing suggestion to any enthusiast who may be precluded by circumstances from undertaking first-hand experimentation.

## Part II b. Principles of Scoring Hits

## 31

I must now discuss the question of what principles should be adopted, and how much latitude, if any, we should allow ourselves, in the matter of counting hits in doubtful cases.

It is necessary to do this at what, in a sense, is a wholly disproportionate length: necessary, because otherwise many experimenters will almost certainly count hits where they should not, and thus tend to generate spurious results; disproportionate, because the actual percentage of doubtful cases is small in practice, as in the matter of Catalogue entries.

Logically, the position seems to be broadly as follows: The experimenter draws an original having certain attributes  $a_1, a_2, a_3 \dots$  etc., and our business is to enquire whether the production by the percipients of drawings having certain attributes  $b_1, b_2, b_3, \dots$  etc., is independent of this fact and such as may be attributed to chance alone, or whether there is observable a non-chance relationship, such as is commonly accepted as a causal connection, between the two events; in particular, we enquire whether, if the original is drawn at time  $t_0$ , and  $p_{bt}$  is the probability of a drawing having the 'b' attributes occurring at time  $t$ , the value of  $p_{bt}$  is independent of  $t_0 - t$ . To do this for any particular experiment in which the original is used we must ascertain, first, the number,  $h$ , of the  $n$  percipients of that experiment who make drawings having the 'b' attributes and, second, the number,  $c$ , of some sample of  $N$  percipients, who make drawings having these 'b' attributes when the original is not used. If we cannot count the numbers  $h$  and  $c$  with precision our enquiry will be invalidated.

The first point to note here is that it is not logically necessary for all, or indeed any, of the 'b' attributes to be identical with all or any of the 'a' attributes. That is to say, we are perfectly entitled, so far as the logic of the method is concerned, to enquire whether the use of an original representing a TIGER prompts a non-chance production of drawings representing lighthouses; it is only common sense—which I suppose means here our accumulated experience of more or less relevant phenomena generally—which suggests that it will probably be more profitable to test for the production of tigers. Equally, we are logically entitled—though it would hardly be more sensible—to go to the opposite extreme and interest ourselves only in cases where every attribute that could possibly be predicated of the original should also be predicable of the drawing—i.e., insist on a perfect point-to-point correspondence (such as could never occur in practice) between original and drawing before admitting a hit.

But whether we adopt either of these fantastic extremes, or some more reasonable standard between them, it is always imperative

that we should be able to ascertain exactly not only the number of percipients making drawings with 'b' attributes in the experiment under examination, but the number who have done so in the sample with which the experiment is compared, i.e., among the percipients of the Catalogue; we must be able to enumerate *all* drawings having 'b' attributes which have been drawn by these percipients. If we cannot do this, assessment becomes impossible and drawings of the type in question may not be counted as hits.

## 32

The next thing to note is that, as common sense would suggest, so experience confirms that even approximately facsimile reproductions of the original seldom if ever occur. An original of a left HAND, for example, drawn with fingers spread and palm downwards, is likely to elicit a number of hands exceeding expectation; but, as I observed in my first paper (1. p. 130), "it is as if the percipient were told 'draw a hand' rather than 'copy this hand,' for we get left hands and right hands, open hands and closed hands, apparently quite indiscriminately"; in other words, it seems to be the 'idea' or content rather than the visible *shape* or form of the original drawing that is cognized, and then set down in whatever form happens to appeal to the percipient.

I do not propose to discuss in detail here just what is to be understood by the word 'idea,' but we may take it to refer, in the case of any given person at any given time, to that group of images which is brought into or nearer the center or focus of the field of consciousness by any appropriate stimulus, such as mention of the name of an object; in most cases, I suppose, this will more particularly involve a visual image or images of some typical example of that class of object, or of one which has impressed itself with especial vividness on the mind concerned. But inasmuch as different people have different experiences, while it is from these experiences that their ideas of objects are derived, it is wholly natural that A's idea of a hand should be different from B's idea. Thus the view that drawings of hands, etc., in different positions, etc., are equally to be regarded as hits on an original HAND, involves nothing at all implausible, but rather the contrary, unless we commit ourselves in advance to the theory that the percipient *must* in some sense 'see' the original and copy it. This would be most improper from a logical point of view, and even if it were true that the percipient 'saw' the original he might still prefer to render it in a somewhat different form.

Thus the first principle in scoring hits is that "An X is always an X"; that is to say, a drawing of any object is always a hit on an original of the same object, regardless of the position, etc., in which it may be shown. A cat is a cat, whether standing, sitting, lying, or running, and a motor-car is a motor-car, whether facing right or left, towards or away from the observer.

The second principle, which is even more important and should be graven deep on the heart of every experimenter, is "A Y is *not* an X," and in general, unless a drawing plainly and unambiguously represents the same, or substantially the same, object as the original, a hit may *not* be scored.

It is, of course, through the small loopholes provided by the words 'in general' and 'substantially' that all our difficulties enter. They may, I think, be completely excluded by laying down and adhering to a categorical and inflexible rule that only the most direct and literal hits shall ever be counted at all—provided always, in view of the preceding section, that the literality is of idea or content and not of form. For my own part, I have no quarrel at all with anyone who chooses to adopt such a policy of ultra-literality, which can only retard and not prevent the emergence of significant results; for I am very much more afraid of uncritical enthusiasts discrediting the technique in particular, and thereby the subject as a whole, by claiming illegitimate hits, than of purists who, at worst, can only waste a certain amount of valuable material by rejecting hits which might reasonably be accepted. Indeed, so long as any given experimenter is in doubt as to the reality of the phenomena in general, and is concerned primarily with strict tests of their occurrence, a policy of literalism can hardly be too strongly advocated; and even those who are confident that paranormal cognition does occur should be extremely cautious until they have gained considerable experience, and should relegate doubtful cases to the cold-storage of the note-book rather than draw hasty conclusions from them.

The question then arises, "When is an X not an X?" I think the answer must be based on the simplest and most common sense considerations, and particularly on whether the names of the two objects in question are commonly held to carry the same connotations. Any cat, in any position, etc., is a hit on CAT but a tiger is not a cat except to zoologists, and it is mere quibbling to pretend that it is; and a polecat has such different connotations from cat that I should not count it as a hit. A horse is a horse, and so is a mention of 'two horses,' because the idea of horse must have been very much in or near the percipient's mind; but a donkey is not a horse, nor is a mule, though I think a pony might be. An ironing board is not an iron, a saw-horse is neither a horse nor a saw; "a sinking ship" is not a rowing boat, though I think we should rather doubtfully concede "a rowing eight"; a cap is neither a hat nor a helmet, but I think "a steel cap" would score on the latter. On the other hand, an alligator certainly is a crocodile, for both the denotations and connotations are indistinguishable for most ordinary people; but a lizard is not. Homonyms, of course, are entirely out of court, except for *ad hoc* research; that is to say, we should certainly never count, in the first instance, an archery bow as a hit on a BOW of ribbon, though we might think it worth while to inquire whether the use of the one tends to elicit the other.

I think there are only two possible kinds of exception to this rule, to show that most superficially doubtful cases can be easily resolved on common sense lines. Sometimes, however, cases of real difficulty are bound to arise; for instance, is a flock of sheep a hit on SHEEP (single)—or is a ram, or a lamb? I should say myself that a ram is, on the ground that most people do not distinguish ram from sheep in their thought; but that lamb is not, because it carries distinct connotations (e.g., of spring, friskiness, etc.) of its own. I should also rule against a flock of sheep, at least for test purposes, partly because it seems to me connotationally different from a single sheep, but mainly for the sake of consistency; for I should certainly not regard a wood or forest as a satisfactory hit on a single TREE. But such genuinely doubtful cases as this can always be dealt with by invoking the principle of literalism to ensure safety.

## 34

It follows from what has been said above, notably in section 31, that hits must seldom if ever be scored on the ground of the shape or form of the drawing alone, i.e., on the ground that although the drawing represents a Y (or perhaps nothing identifiable) it looks more or less like an X, as a drawing of a pond fringed with reeds might look rather like a crown. The reason is that we have no means of telling how many drawings which might equally plausibly be held to look like X have been drawn by the percipients of the Catalogue, so that the essential basis of assessment is lacking. This rule is extremely important and must be closely adhered to, though it bears hardly on certain percipients (and these, by inspection, by no means the least gifted) who seem correctly to cognize the form of the original, or part thereof, without being able to identify the content. I do not think that this means that they in any sense 'see' the original, but rather that images of the marks made by himself on the paper must form part of the experimenter's contemporary 'idea' of the object he is drawing—i.e., must be associated with the other images constituting that idea—and that percipients of this type happen to select these images in preference to others by reason of some idiosyncrasy of their own.

Sometimes this rule operates extremely harshly: For example, in Expt. VIII, HK, mentioned below, the experimenter illustrated the word INCLINE, as an original, by a sketch of a car ascending a steep hill, and a percipient drew a skier sliding down a steep slope, both sketches being markedly diagrammatic. The temptation to award a hit was almost irresistible, but I could not accept it at its face value, because the Catalogue does not show the number of steep slopes drawn by its 741 percipients.

I think the foregoing examples, few though they be, should serve namely (a) if the shape of the object depicted in the original is so distinctive that no drawing of substantially the same shape could humanly speaking, be plausibly held to represent anything else and would certainly have been catalogued as that object if it had occurred,

or (b) if we have antecedent evidence to the effect that percipients tend to draw object A when object B is used as an original.

As an example of the first, I recently conceded a hit on an original GLOBE (as used for geography) to a drawing which appeared unmistakably to represent a sphere mounted on a vertical stem carried by a circular base, the proportions being approximately those of the usual globe. But even here, although to deny the hit would have been to carry prudence to a preposterous length, I should have felt some qualm of conscience had I not been confident that, if any similar drawing occurred among those catalogued, it would have been duly listed, probably under 'GLOBE, very approximate shape,' as was done for the two quasi-catapults, *q.v.* This is, however, an extreme case, and I mention it only to show how close the correspondence between drawing and original must be before the award of a hit on a basis of form, without certainty as to intended content, becomes permissible at all.

As for the second, the most striking example is the outcrop of hour-glasses in Expt. VI, which I have little doubt were determined by the original bow, drawn as a bow tie, used in that experiment, and I strongly suspect that the same is true of the milk bottles and THERMOS of Expt. VII. In the former case at least there is not only a strong resemblance between the outlines of the two objects, but the excess of hour-glasses over expectation is significant. Consequently, if in some future experiment I were to use bow (tie) as an original, I should feel entitled to count as hits any hour-glasses the percipients might draw (and the same, to a less extent, with THERMOS and milk bottles), but it would be incumbent on me, if I so decided to add the net frequency for hour-glasses (*viz.*, 5) to that for BOWS (*viz.*, 4) in determining the value of *c*, regardless of whether hour-glasses were drawn or not; for it would clearly be unfair to give ourselves the benefit of them if they were drawn while evading the penalty, so to say, if they were not. The same principle must be applied, of course, in all similar cases; that is to say, whenever for any reason at all we decide that it is proper to count an X', say, as a hit on an original X in determining *h*, the corresponding net catalogue frequency of X' must be added to that of X in determining *c*.

On the other hand, if HOUR-GLASS were employed as an original, I should use only the frequencies under I-V and VII in determining *c*, for the occurrences in VI are at least strongly suspect of being due to BOW, and thus not attributable to chance alone; and it is clearly imprudent to use suspect data if others are available. This, however, should not be done in other cases *merely* because one of the three pairs of frequencies shows some considerable excess over the other two, for a certain number of such excesses is to be expected by chance in a sufficiently large sample of objects; it is permissible only if the excess is significant *and* there is some plausible suggestion that the occurrences have been influenced by some identifiable factor.



At first sight there may appear to be some contradiction between the remarks of the two preceding sections and those of section 31, where I pointed out that we are perfectly entitled to test whether there is a non-chance connection between the use of any original and the occurrence of any type of drawing whatsoever; but this is not the case, for section 31 deals in this connection with the abstract logic of the matter, which is not quite the same as the practical methodology. We are perfectly entitled to decide, *before* we see the material, to count any specified sort of drawing as a hit on any original, or to enquire, after we have seen it, as a matter of interest, whether any sort of drawing occurs in excess of expectation; what we are *not* entitled to do is to look through the drawings and pick out as hits anything that happens to strike our fancy and looks as if counting it would be profitable. To allow this would be to open the door to every kind of wish-thinking, which we must above all things avoid, and that is why I have insisted on literalism of content as the basic principle which should guide us in scoring hits.

I should, however, be sorry if this insistence were to be taken as implying that we should never exercise our discretion or permit ourselves a judicious latitude in our scoring. There seems little doubt that an original may in actual fact be responsible for the production of drawings not quite identical with itself even in content, and to ignore these altogether in the interests of safety at all costs would be to waste valuable material. Moreover, it is not always possible in practice to foresee what form such aberrations may take, so that the experimenter may well find himself confronted with a drawing which he feels it would affront reason to deny as a hit but which he has not decided in advance to accept, simply because he has not thought of it. This kind of thing might lead to his obtaining too low a score, which, as an open-minded investigator, he would presumably deplore only less than a too high one.

The fact of the matter is, I think, that our scoring policy, and particularly the degree of latitude we permit ourselves in deviating from strict literalism<sup>1</sup> should depend on what it is that we are trying to do. This may range from adducing evidence which shall be proof against any rational attack of even hostile critics, to finding hints that may throw light on the psychological processes involved or suggest future experiments; for the first purpose we can hardly be too literal, for the second, even relatively far-fetched possibilities may be provisionally entertained. But in general, and especially for such routine purposes as studying the relative merits of different classes of percipients, sizes of experiments, or types of original, we want the best balanced and most accurate results that a conscientious study of the data can give us; and these will not be obtained by a too drastic rejection of apparent hits in accordance with inflexibly applied rules.

<sup>1</sup> The qualifying words 'of content' are here and hereinafter taken as understood.

There is, in fact, an important practical difference between what I have termed 'test' and 'informational' scoring. For the first, that is to say for the purpose of obtaining virtually critic-proof evidence, we must keep almost blindly to the strictest rule of literalism, because this is our only safeguard against allegations to the effect that our scoring has been biased by our inclinations; but for the second we should allow ourselves such reasonable latitude as common sense tempered by caution may suggest, just as we do in any other field of scientific inquiry. We shall, to be sure, occasionally make mistakes, sometimes in one direction and sometimes in the other; but this is true of any work whatever into which any kind of judgment enters, and is no reason for invariably binding ourselves by the dictates of a rigid formalism.

## 36

There are, I think, two principal types of situation in which a certain latitude may reasonably and properly be allowed.

The first is when an object fails to conform to the rule of literalism more by an accident of nomenclature, so to put it, than because of any real difference of content or idea between it and the original. Thus a chair of notably ceremonial appearance or function (e.g., a presidential chair) is scarcely distinguishable from THRONE by any reasonable criterion, and it seems barely practicable to draw a hard and fast line between tent and wigwam, though these several objects have different names and appear in the Catalogue under different headings; but I think they should be regarded as virtually synonymous for informational purposes. Sometimes, of course, the experimenter is forced to treat two or more distinguishable objects as indistinguishable simply because they are not separately listed in the Catalogue (e.g., open and closed motor-cars, cats and kittens); but this, even if he may disagree with the arrangement, can do no harm methodologically since it is in the nature of an arbitrary convention applied to all percipients alike.

The other type of situation, which is commoner and more interesting, is where the original represents an object very rarely drawn by percipients, and of a character, therefore, which does not come readily to mind in the context of the experiment. In such cases it seems to me perfectly legitimate, for informational purposes, to accept 'near misses' or 'nearest approaches,' provided they are genuinely 'near,' and show strong natural affiliations with the original by straightforward common sense standards. Thus, no Catalogue percipient actually drew or mentioned a PRAWN or a BUFFALO, though one got so far as buffalo horns; but I have no hesitation—for informational purposes, I repeat—in accepting lobster or crayfish, for the first or bulls, cows, etc., for the second. Indeed, it seems to me that it would be definitely foolish to do otherwise; for, if we consider how many of the images which in association make up the idea of lobster, say, must be closely similar to, if not identical with,

those that make up the idea of prawn in the minds of most people, it is clear that the presence of the one is almost a guarantee that the other is very close; and this, assuming it to be a legitimate inference, is really much more important than the way in which the percipient actually happened to formulate his thought. Needless to say, however, the utmost discretion should be exercised in such cases.

One small practical point remains to be mentioned, namely that if, for any reason, the original is not a simple object, but resolvable into two or more components each of which might form an original by itself, then these components should be treated as separate originals in all respects. For example, the ROYAL STANDARD of Expt. II consisted of a flag *plus* a lion, and the FLAG of Expt. IV of a flag *plus* a cross (the cross being the most striking feature), and I have acted accordingly, treating each as two originals, flag and lion and flag and cross, in the scorings of section 37 below, and similarly in one or two other cases. In the interests of simplicity, however, such composite originals should be avoided.

To sum up: For all test purposes the policy of literalism of content should be resolutely adhered to, even though it means that the experimenter will have to do rather more work in order to obtain a significant result; but for informational studies a certain amount of latitude is permissible, on the lines indicated, subject to the exercise of great discretion. The experimenter should, I think, invariably indicate whether his scoring has been strictly literal or not, and, wherever practicable, the nature and importance of his deviations from it, if any.

### Part III

#### The Technique in Action

##### A. Experiments I - VII

###### 37

It would be as imprudent to proffer a technique for general use without ascertaining the kind of results it gives in practice, as to put a novel type of automobile on the market without adequate tests on the road; and experimenters can hardly be expected to embark on an even mildly troublesome investigation unless they have some fairly good notion of their prospects of success.

To a certain extent this preliminary testing can be retrospective; because, on the purely operational side, I have but trifling modifications to suggest in the procedures described in my earlier papers, while the method of assessment may as usefully be tested on existing data as on fresh. We may accordingly begin our review of the technique in action by using Fisher Scores and the Catalogue data to assess the results of the first seven experiments; and this will have the added advantage that it will exhibit their relative degrees of success in a way which the earlier cross-scoring necessarily failed to do.

The results of doing this are given in Tables Ia and Ib below. For the first of these the most rigidly literal methods possible have been followed—indeed, literalism has been carried to almost fantastic extremes, as by not admitting buffalo horns as a hit on BUFFALO, or field guns, etc., for SHOOTING; and the scoring adheres strictly to the indications of the Catalogue, i.e., only those objects are counted as hits which are there specified as having been used as originals—for example, only hemispherical bowls are counted as hits on the BOWL of VII, E. In the second, I have allowed a modest latitude, in accordance with the principles discussed above, e.g., counting cows, etc., for BUFFALO and flocks and lambs for SHEEP; but even here the utmost discretion has been exercised—for example, I have not counted hour-glasses for BOW in VI, though I think it would probably be perfectly legitimate to do so. The figures in Ia have been computed by the ultra-conservative procedure shown in the first part of the Example, but those of Ib by the multiple unit' method normally used. In dealing with Expts. I-V, I have been guided by the considerations mentioned in section 11 above; that is to say, I have taken N as 491 in all cases, and have made corresponding deductions, in determining the values of  $c$ , of all frequencies in the first two columns of the Catalogue from the gross Catalogue frequencies.

The values given under the heading of 'Mean Scores' are Mean Scores per percipient per original, with corresponding variances.

That is to say, the mean scores are obtained by dividing the actual scores by the product  $ns$  in each case, where  $n$  is the number of percipients taking part in the experiment and  $s$  is the number of originals used, and the variances by dividing the actual variances by the squares of these products. A compound original split into two or more components, in the manner discussed in the preceding section (as has been done wherever circumstances required it) counts as two or more originals.

These means enable us to judge of the relative degree of success of the various experiments regardless of the number of percipients taking part in them or the number of originals used; other things being equal, the actual scores would, of course, be approximately proportional to the number of percipients and the number of originals.

Weighted means are obtained by weighting the contributions inversely as their variances in the usual way.

The indications of recent work, not yet ready for publication, are to the effect that the low scores for Expt. VII are due to the fact that the percipients of this experiment were not provided with a photograph showing the surroundings, etc., in which the originals would be produced and displayed, as was done in Expts. I and III-VI.

TABLE Ia.

## LITERAL SCORING

Expt.	$n$	$s$	Actual Scores		Mean Scores		$D/\sigma$	$\sim P$
			Score	Vce	Mean $\times 10^3$	Vce $\times 10^6$		
I	37	12	7.212	14.619	16.243	74.157	1.89	.06
II	20	14	12.895	12.325	46.054	157.206	3.67	.001
III	11	10	.426	5.267	3.873	435.289	.19	.85
IV	105	11	11.972	62.071	10.365	46.529	1.52	.13
V	77	10	.746	45.664	.969	77.018	.11	.91
Total I-V	250	57	33.251	139.946			2.81	.01
Weighted Mean I-V					13.363	17.675	2.86	.01
VI	246	10	34.558	125.444	14.048	20.729	3.09	.005
VIIA	245	10	3.868	98.636	1.579	16.433	.39	.70
VIIIB	"	"	27.551	102.933	11.245	17.148	2.72	.01
VIIIC	"	"	5.285	97.550	2.157	16.252	.54	.59
VIIID	"	"	12.253	106.124	5.001	17.680	1.19	.23
VIIIE	"	"	4.301	116.920	1.756	19.479	.40	.69
Total VII	"	50	53.258	522.163	4.348	3.480	2.33	.02
TOTAL I-VII	741	117	121.067	787.553			4.31	$\sim 10^{-5}$
Weighted Mean I-VII					6.841	2.550	4.28	$\sim 10^{-5}$

TABLE Ib.  
INFORMATIONAL SCORING

Expt.	n	s	Score	Vce	Mean	Vce	D/σ	—P
I	37	12	17.030	17.932	38.356	90.963	4.02	10 <sup>-4</sup>
II	20	15	24.484	14.790	81.613	164.333	8.36	10 <sup>-9</sup>
III	11	10	4.370	5.611	38.818	463.719	1.80	.07
IV	105	11	17.736	61.019	15.365	45.741	2.27	.03
V	77	10	9.520	46.445	12.364	78.335	1.40	.16
Total I-V	250	58	73.040	145.797				
Weighted Mean I-V					27.770	18.565	6.43	10 <sup>-9</sup>
VI	246	10	52.182	124.565	21.212	20.584	4.67	10 <sup>-5</sup>
VIIA	245	10	6.538	99.481	2.670	16.633	.65	.51
VII B	"	"	48.074	107.013	19.622	17.828	4.65	10 <sup>-5</sup>
VIII C	"	"	22.101	119.227	9.021	19.863	2.02	.05
VIII D	"	"	28.126	112.558	11.480	18.752	2.65	.01
VIII E	"	"	9.897	119.805	4.040	19.960	.90	.37
Total VII	245	50	114.736	558.444	9.336	3.721	4.86	10 <sup>-6</sup>
Total I-VII	741	118	239.958	828.806			8.35	10 <sup>-9</sup>
Weighted Mean I-VII					13.585	2.694	8.27	10 <sup>-9</sup>

## 38

I must here make a digression of considerable theoretical interest concerning a point, which, if it had been correctly formulated, would have constituted an important criticism of the whole technique, irrespective of the method of assessment employed, or of almost any other depending on the use of 'free' material such as drawings.

I think this may be stated with sufficient accuracy as follows: Whatever method of assessment we may adopt, we are concerned essentially with the question whether certain events—notably the production by the percipients of an experiment of a certain type of drawing—occur more often than we should expect on certain assumptions—notably that they will tend to produce them with the same relative frequency as the percipients of the Catalogue. But we also assume, in each case, that these events in which we are interested occur independently of one another except insofar as they may be influenced by paranormal cognition or like cause which we are concerned to investigate; and insofar as this assumption is incorrect our conclusion is likely to be invalidated. Thus, if any factor whatever, other than paranormal cognition, so operates as to cause two or more percipients to draw the same object, used as an original, where only one (or none) would otherwise have done so, we may find ourselves unknowingly counting as plural and independent a group of events which were in fact no more than one single event, to wit the operation of the factor postulated. This type of error,

which may be termed the *pro pluribus unum error*, has been and still is all too common in attempted applications of probability theory, from the time of Condorcet onwards, and we must be most careful to make sure that nothing of the kind is responsible for the results obtained in this work.

Errors of this type might arise in many different ways. For example, it may safely be premised that the relative frequencies with which different objects are drawn will not in general be the same for the two sexes. If, then, an experiment be performed in which there is a somewhat higher proportion of women than in the Catalogue, and if the originals used in that experiment happen to be somewhat above the average as regards their liability to be drawn by women rather than by men—which, on a random selection of originals, is an even chance—it is likely that a certain number of hits may be scored, not because the percipients concerned receive separate and independent impressions of the object represented, but because they are women—i.e., by virtue of a common factor operating upon them—and the resultant score will be somewhat too high. On the other hand, if the proportion of women in the experiment be lower than that for the Catalogue, or if the originals happen to be relatively less 'popular' with women than with men, then the effect will be in the opposite sense and the score will be somewhat too low. Precisely similar considerations apply to differences of nationality, or of age, or of any other attribute that can be predicated of percipients.

## 39

Closely allied to the foregoing—indeed, identical from the statistical standpoint—is the possible influence of 'topical interest' as ordinarily understood. One might reasonably argue, for example, that if DOG happened to be used as an original at a time when a dog show was in progress, percipients would tend to draw dogs more freely than they would otherwise have done, and that this would lead to spuriously high scores. This would be another case of the *pro pluribus unum error*, because we should be treating as plural and independent a number of events, namely drawings of dogs, which were really no more than manifestations of one single event, namely the holding of the show, operating on several percipients.

More generally, it would be perfectly correct to maintain that whereas the method assumes that, in the absence of paranormal cognition, the probability that any object will be drawn by a percipient is constant—in particular, the same for the experiment as for the Catalogue—this will never in fact be precisely true, and that the calculated score will always be somewhat too high or too low according to whether the average probability for the originals used happens to be above or below that given by the Catalogue. Thus it is only necessary for the critic to maintain, in the case of any given experiment, first, that the average probability 'just happened' to be higher than was assumed and, second, that the effect of any such



happening may be a good deal bigger than we suspect, in order to account for the emergence of a positive result without invoking paranormal cognition at all; and he can do the same, *mutatis mutandis*, for the factors discussed in the preceding section.

Criticism on such lines as these could easily be made to appear extremely formidable, nor can it be refuted merely by arguing against it. Common sense and study of the material combine, it is true, to assure us that such factors as these are likely to operate, and in practice do operate, only to a quite inappreciable extent; but to affirm this is merely to express an opinion which, logically, the critic is entitled to repudiate. We may think it ridiculous to suppose that the percipients of the first experiment were affected by a passing wave of cow-consciousness, or that those of the fourth drew crosses in a sudden fit of religiosity, but we have no remedy if the critic chooses to aver that he finds such hypotheses much more plausible than that of paranormal cognition; and in general we cannot base our claims on a belief, however reasonably held, that the effect of any such factor can only be 'a very little one.'

We can, to be sure, meet suggestions about differences of sex and nationality, etc., by showing (as happens to be true) that women alone or British subjects alone give a significant result, and that therefore the significance found cannot be due to varying proportions of these; but then the critic can shift his ground to varying proportions of blue-eyed, French-speaking, oyster-loving, etc., percipients, and can continue the game indefinitely.

All particular criticisms of this general type can, however, be met simultaneously by pointing out that, of whatsoever nature and however numerous may be the factors invoked, their combined effect is as likely to be in one direction as in the other in any particular case, provided the originals are randomly selected; that is to say, it is an even chance whether they will, on balance, operate *in favor* of the percipients or *against* them. Thus, although the critic can logically, if not very plausibly, ascribe the positive outcome of a single experiment to the chance operation of such factors, he cannot do so for a succession of experiments, or for a significant majority of a number of them, without affirming that chance operates, in greater or less degree, consistently in favor of the percipients; and this is a contradiction in terms.

But the results given in Table I show that, even under the most rigorously literal scoring, every one of the eleven sets of ten originals each dealt with therein shows a positive result. The probability of such a sequence of eleven results of the same sign occurring by chance, is, of course, 1 in 1,024.

We may accordingly dismiss with assurance, so far as this work is concerned, counter-hypotheses based on suppositions to the effect that the results obtained have been due to fortunate coincidences of originals with favorable fluctuations in their 'popularity' due to variations either of topical interest or of the constitution of the participating percipients. This does not imply, however, that no

particular experiment can ever be influenced, in either a positive or negative sense, by topical or like factors; and, in general, experimenters should remember that, other things being equal, a small quantity of material, or short series of experiments, can never yield evidence so reliable or so cogent as a large quantity or a longer series. On the other hand, just as was the case with sex, etc., preferences, inspection suggests that topical influences are so small as to be negligible.

## B. Experiment VIII

### 40

Under the general heading of 'Experiment VIII' I subsume the work of 25 different experimenters, conducting independent experiments, between November 1940 and February 1942 inclusive. Two of these, 'PG' and 'RH,' did two experiments each, while several of the other experiments consisted of two or more parts. For convenience of tabulation, etc., I also include the Wing experiment already discussed, which was of the same independently conducted type though performed at a much earlier date.

Although the work was both protracted and laborious, my account of it must be exceedingly abbreviated, but this is no disadvantage since none but trivial alterations were made in the procedure previously adopted. These will, however, be duly noted below.

The work was initiated in pursuance of the policy of attempting to develop a repeatable technique for general use, with the object of seeing how experiments with drawings fared in hands other than my own, and particularly of ascertaining whether the method of assessment by cross-scoring was usefully applicable to individual small-scale experimentation. With these objects in view, I approached, in the summer of 1940, a considerable number of people, some in the British Isles and others in the United States of America, who I thought might be willing to collaborate, and invited them to try experiments on these lines. I must here express my deep gratitude to the following for the help they have given me, and for the time and trouble they devoted to the work:<sup>1</sup>

In U. K., Dr. J. M. Thorburn (Cardiff); Miss Marjorie Hammond (Birmingham); Dr. P. E. Vernon (Glasgow); Mrs. Oliver Gatty, Dr. C. M. Fleming (London); Miss Estelle Canziani, Mr. G. le M. Mander, M. P.; Mr. G. H. Spinney; Dr. M. I. Balfour; Miss Theodora Bosanquet; Mrs. Frank Heywood; Mrs. W. Kneale

<sup>1</sup> Names are given in approximately the chronological order of the experiments. In the case of University teachers, etc., the name of the University follows in parentheses.

(Oxford); Dr. Mary Collins (Edinburgh); Mr. J. Fraser Nicol; Mr. Kenneth Richmond; Viscountess Traprain; Dr. Ll. Wynn-Jones (Leeds); Mrs. M. E. Bosanquet.

In U. S. A., Dr. C. M. Harsh (Nebraska); Dr. D. M. Allan (Hampden-Sydney); Mr. E. P. Gibson; Dr. H. Kettering (Colorado); Dr. George A. Zirkle (Hanover, Ind.); Dr. John Bentley (American Univ., Washington D. C.); Dr. Gardner Murphy (College of the City of New York).

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Experimenters were supplied with books of foolscap size, on the pages of which they were to draw the originals, very similar to those used by the percipients of earlier experiments, and bearing instructions as to the conduct of the experiment printed on their covers. Three alternative types of experimental procedure were suggested, namely (A) a ten-evening experiment precisely similar to my own I, III, IV and V, (B) a group experiment, with suitable invigilation or equivalent, similar to my number II—i.e., with all percipients present together and the work completed in about an hour, or (C) an intermediate type in which the percipients make their drawings in their own homes while the experimenter works to a prearranged time-table, producing originals at, say, five-minute intervals. The type of experiment adopted is shown in Table II under the heading T.

Experimenters were urged to do one or more repetitions of the experiment wherever possible, or at least to prepare, immediately after the experiment proper, another series of ten 'dummy' originals for comparative purposes. Actually, six experiments (Nos. 2, 9, 22, 23, 24 and 26) had two complete parts each; three (Nos. 8, 17 and 25) had three parts, one of them (No. 8) having dummies for the first part also; one (No. 27) used 30 originals in a slightly different procedure, and this may be regarded as equivalent to a three-part experiment; one of the two-part experiments (No. 26) had dummies to both parts; and of the remaining 17, ten had dummies in addition to the originals proper, while seven had none. The object of these 'dummy' originals (somewhat unwisely referred to as 'controls' in my instructions, though they could not, of course, 'control' anything) was partly to add to the interest of the work from the percipients' point of view, but mainly to provide 'straight' precognitive material for examination in cases where no second part was attempted.

In general, experimenters were directed to use the 'dictionary' method of selecting originals; that is to say, to insert ten numbered and shuffled cards randomly into a dictionary, to open this at the pages so indicated in the order given by the numbers, and to illustrate as an original the first readily drawable word found. In experiments 17, 23, 24, 26 and 27, however, I supplied the experimenters with a number of folded slips of paper on which I had myself written words

taken at random from the Catalogue, from which they in turn drew ten at random to determine what the originals should be. I shall have more to say about selection of originals in section 44 below, but it may be noted here that the 'dictionary' method leaves much to be desired. This is not because experimenters show any tendency to evade difficult and choose easy (and possibly topical) objects, but rather because, in an excess of conscientiousness, they try to illustrate objects which are too ambiguous, recondite or difficult of recognition to be suitable for use as originals at all.

In the interests of paper economy, I issued percipients with double foolscap sheets for their drawings, instead of ten-page books. The first sheet carried instructions, while all ten drawings were done on the second, which was divided into ten rectangles measuring about  $3\frac{1}{2}'' \times 2''$ . This seems to have had no ill effects, but rather to have been beneficial in discouraging the production of vague 'scenes' and the like.

The instructions to percipients were substantially the same as those mentioned above, with one fairly important alteration. Instead of telling them to 'reproduce' the original, or to draw the object they thought it represented, I said, "You are asked to *write down*" (my italics, not in instructions) "what you think each of these originals represents. Add a small sketch of your own if you can." The object of this alteration was mainly to avoid ambiguities and to lighten the task of the scorer by reducing the number of cases in which the percipient's intention might be doubtful, but partly also to encourage those who could not draw, and to promote concreteness; I also wished in some degree to direct attention away from mere shape or form and towards the content of the original. So far as I can judge, this alteration has been beneficial, at least as regards the first point, and I have noticed no concomitant disadvantages. It may be noted that the great majority of percipients—fully 90%, at a guess—did in fact attempt drawings of some kind, even when they manifestly could not draw.

The importance of excluding all possibility of normal cognition was, of course, suitably impressed on experimenters, who were asked to sign a declaration to the effect that this had been done. Each percipient was also asked to sign a similar statement and one affirming that he "was not wittingly influenced by what anyone else put down." In a few cases, where these signatures had been inadvertently omitted, I took appropriate steps to satisfy myself that all was in order; in a few others, where I could not do this, the percipients concerned were omitted from calculation.

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At the time when I initiated this work I had not realized the possibilities of assessment afforded by the use of Fisher Scores, and I thought that I, and future experimenters, would have to rely on cross-scoring to obtain our results. I hoped, and more or less expected, to find that if an experiment were conducted in two or more

parts, separated by a suitable interval or intervals of, say, three weeks or a month, and all drawings were then scored against all originals, we should obtain a positive result (not necessarily significant) often enough—say, seven or eight times out of ten—to make the procedure a worthwhile method of research. In this I was wrong, for I had clearly underestimated seriously the extent to which 'displacement' or 'temporal dislocation of response' may take place between the parts of an experiment (and also, it would appear, between different experiments).<sup>1</sup> It seems fairly clear to me now that astronomical time *per se* is not the only factor determining this effect, so that it is not practicable to specify any reasonably short interval between the parts of an experiment such that we may be confident that pre- and retro-cognitive effects will not appreciably operate, so to say, across it.

I accordingly arranged for the experiments to be scored in pairs by external scorers who did not know which originals had been aimed at by which percipients. I am very much indebted indeed to Mr. G. W. Fisk, Mr. Fraser Nicol, Mr. Kenneth Richmond, Major K. H. Tuson and Bishop F. J. Western for their kindness in undertaking this most tiresome work, which is not rendered the less valuable by the fact that it yielded virtually null results.

This nullity of outcome, however, relieves me of the necessity for describing in detail the methods I adopted for ensuring an arbitrary pairing of the experiments to be scored against each other—a task much complicated by the erratic and belated arrival of some contributions—or the devices, some of them elaborate, which I used in many cases to ensure that not even the remoter sources of error should have a chance to vitiate the results. It will be understood that, in each case, the suitably randomized drawings of all percipients of both experiments were scored against all the randomized originals of both, and the data computed in a 2 x 2 table in the usual way: similar treatment was applied to different parts of single experiments, in cases where more than one part had been done.

Twelve pairs of experiments were cross-scored in this way, yielding eight positive and four negative results, while of the nine multi-part experiments among them six were positive and three negative. All that can be said of results like these is that they are in the right direction, though falling far short of significance; and it became clear that either cross-scoring was too insensitive a method to be of much use for general purposes, or else the degree of interaction between experiments, or parts of the same experiment, was likely to be so great as seriously to obscure the effects looked for.

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Meanwhile, however, the method of assessment by Fisher Scores and Catalogue data was developed, and all the material of Expt. VIII has now been scored in this way. The standard of scoring adopted

<sup>1</sup> Cf. Note to section 11 above.

approximates very closely to that use for Table I a.—that is to say, almost perfectly rigid literalism—although I found it necessary to exercise an occasional modicum of discretion in respect of some of the less straightforward originals; on the other hand, in the interests of a conscientious conservatism (if this be not a contradiction in terms) I have very frequently rejected hits which by all common sense standards would unquestionably be accepted.

The results are given in Table II, which is to be interpreted as follows: The first column gives the number of the experiment in approximately chronological order, a central date being taken in cases of multi-part experiments; the second shows the identifying letters of the experimenter (usually initials) followed by a number denoting the part of the experiment referred to; the third and fourth give the number of percipients in that part, and the type of experimental procedure used. As regards the three main vertical divisions, the figures under 'Contemporary' give the scores and variances for hits made by the percipients on the originals at which they were aiming, regardless (as usual) of whether the ordinal position of the drawing scoring the hit is the same as that of the original on which the hit is scored; those under 'Precognitive' refer to hits made on originals used in later parts of the same experiment, or on the 'dummies' referred to above; those under 'Retrocognitive' refer to hits scored on originals (including 'dummies') of earlier parts of the experiment, but these figures have only been computed in the few cases where I knew positively that the nature of the originals of the first part, etc., had not been disclosed before the percipients made their drawings for the second or third part. The precognitive scores on dummies are given before those on the originals of later parts: thus, for Expt. 8, PV, the first line shows, under Precognitive, the scores made by the percipients of the first part on the dummies of the first part, and, under Contemporary, the scores made by them on the original proper of this part; the second and third lines give the scores made by these same percipients on the originals of the second and third parts; the fourth line gives the precognitive score by percipients of the second part on the originals of the third (there were no dummies for the second or third parts) and the 'contemporary' score made by these percipients on the originals of their own (second) part; finally, the fifth line gives the 'contemporary' score by the percipients of the third part on the originals of the third part. A similar arrangement is adopted in other cases.

The Table contains a total of 80 entries altogether, of which 51 are positive and 29 negative; these are distributed as follows:

	+	—	Total
Precognitive .....	18	11	29
Contemporary .....	25	17	42
Retrocognitive .....	8	1	9
Total .....	51	29	80



We may say that experiments of this somewhat mixed type yield positive results about three times out of five, with a noticeable though not significant tendency for retrocognitive data to give higher and precognitive data lower proportions than this. It should be remembered, however, that these experiments have been to some extent handicapped by the use of unsuitable originals, due to the dictionary method of selection; and also that in some cases, where rarely drawn objects have been used and the number of percipients has been small, the total expected number of hits has been less than unity, which means that there is less than an even chance of obtaining a positive result in the absence of paranormal cognition. These 'positive-negative' data are accordingly not very informative.

The unweighted mean score per percipient, for Contemporary data only, is  $76.607/496 = .15445$ , since 496 sets of percipients' drawings were involved, and the corresponding mean variance (which is not, of course, the same as the variance of this mean) is  $231.825/496 = .467389$ .

These figures are important, for they enable us to form some idea of the amount of work the average experimenter will have to do in order to obtain a significant result by his own efforts alone—using the Catalogue, of course, for assessment. It is easy to see that a collection of 100 sets of ten drawings each, of the same average merit as those tabulated in Table II, may be expected to yield a score of about 15.445 with variance about 46.739; this would give  $D/\sigma = 2.26$  with  $P$  less than .03, which is a satisfactory level of significance by ordinary standards. Or, if we adopt the conventional level of  $P = .05$  and  $D/\sigma = 2.0$ , and solve the appropriate equation, we find 78.4 as the number of sets which may be expected to yield on the average a result just attaining this level; while for  $P = .01$  the requisite number of sets will be about 130.

There is no guarantee, of course, that every experimenter who collects about 800 drawings, e.g., by doing four ten-original experiments with 20 percipients in each, will necessarily achieve a significant result, any more than there is that no one who does less will succeed in doing so. Theoretical considerations apart, a glance at the Table would serve to disprove any such notion, for some experiments with relatively large numbers of percipients yield but small positive results, or even negative (e.g., Nos. 2 and 10), while others with small numbers are very successful. But I think we may fairly say that no one who has not collected the better part of a thousand drawings, e.g., five ten original experiments of 20 percipients each, is entitled seriously to complain if his results are not significant.

This is a point worth emphasizing, for I have some reason to believe that several of the experimenters who have so kindly contributed to this work have been disappointed and discouraged at not having been able to detect more overt signs of success than were apparent to inspection; but, apart from the fact that it is strictly impossible to assess the results of an experiment without the aid of the Catalogue, or equivalent body of knowledge, it seems reasonable



TABLE II  
SCORES AND VARIANCES FOR EXPERIMENT VIII.

No.	Expr.	n	T	PRECOGNITIVE		CONTEMPORARY		RETROCOGNITIVE	
				Score	Vce	Score	Vce	Score	Vce
	Wing	19	B	.....	.....	8.097	9.886	.....	.....
1	MT 1	1	C	.....	.....	— .309	.399	.....	.....
2	MH 1	37	B	9.449	19.408	1.493	17.914	.....	.....
	" 2	27	B	.....	.....	3.988	13.554	.496	13.071
3	CH 1	24	C	1.289	11.135	8.869	11.205	.....	.....
4	MA 1	13	B	— .152	6.330	— .219	8.441	.....	.....
5	EG 1	18	C	.142	6.852	.461	5.119	.....	.....
6	HK 1	5	C	2.003	2.262	.063	1.833	.....	.....
7	GZ 1	23	C	5.678	5.654	9.278	10.818	.....	.....
8	PV 1	14	B	1.184	2.520	—1.528	4.838	.....	.....
	" 1	"	"	1.266	4.422	.....	.....	.....	.....
	" 1	"	"	1.478	5.006	.....	.....	.....	.....
	" 2	12	B	— .922	4.297	— .059	3.788	.....	.....
	" 3	"	"	.....	.....	— .035	3.134	.....	.....
9	PG 1	3	B	.949	2.307	2.565	1.578	.....	.....
	" 2	4	B	.....	.....	5.766	3.863	.....	.....
10	CF 1	52	B	—2.957	32.209	—1.061	35.270	.....	.....
11	EC 1	4	C	.....	.....	—1.614	2.100	.....	.....
12	MM 1	2	C	.....	.....	—1.018	1.194	.....	.....
13	JB 1	14	B	.....	.....	9.038	9.940	.....	.....
14	GS 1	5	C	.....	.....	1.575	.901	.....	.....
15	MB 1	4	A	— .117	2.102	1.381	2.682	.....	.....
16	TB 1	5	B	—2.252	2.767	1.637	2.931	.....	.....
17	RH 1	10	A	5.737	5.443	— .633	4.803	.....	.....
	" 1	"	"	.279	4.365	.....	.....	.....	.....
	" 2	10	A	1.244	4.368	— .227	5.565	2.818	4.807
	" 3	9	A	.....	.....	2.306	3.935	.439	4.325
	" 3	"	"	.....	.....	.....	.....	.575	5.036
18	MK 1	12	C	2.069	4.474	— .897	3.971	.....	.....
19	MC 1	22	A	.....	.....	1.252	10.163	.....	.....
20	FN 1	8	C	.....	.....	—2.023	2.823	.....	.....
21	KR 1	6	C	3.083	1.236	4.301	1.804	.....	.....
22	GM 1	5	B	1.063	1.209	5.802	1.321	.....	.....
	" 2	5	B	.....	.....	— .871	1.205	3.837	1.319
23	JT 1	2	C	.178	1.098	— .691	1.033	.....	.....
	" 2	2	C	.....	.....	.178	1.098	.697	1.035
24	WJ 1	12	C	3.011	6.082	3.805	6.475	.....	.....
	" 2	6	C	.....	.....	2.247	3.044	.706	3.237
25	PG 1	3	B	— .591	1.036	2.571	1.532	.....	.....
	" 1	"	"	.669	.897	.....	.....	.....	.....
	" 2	3	B	— .530	.896	— .591	1.036	.....	.....
	" 3	3	B	.....	.....	— .530	.896	.....	.....
26	EB 1	10	B	— .042	6.246	.227	5.091	.....	.....
	" 1	"	"	— .540	4.236	.....	.....	.....	.....
	" 1	"	"	—1.603	4.897	.....	.....	.....	.....
	" 2	10	B	— .217	4.893	— .668	4.236	—1.766	5.140
	" 2	"	"	.....	.....	.....	.....	1.371	6.252
27	RH 1	7	A	.....	.....	4.239	6.312	.....	.....
	" 2	"	"	.....	.....	4.770	6.483	.....	.....
	" 3	"	"	.....	.....	3.672	7.711	.....	.....
TOTALS:				30.848	158.647	76.607	231.825	9.173	44.222
				D/σ 2.45;		D/σ 5.03;		D/σ 1.37;	
				P < .02;		P < 10 <sup>-6</sup>		P ~ .17	
GRAND TOTAL:				Score 116.628;		Vce 434.694;		D/σ 5.59; P < 10 <sup>-6</sup>	

to surmise that, if paranormal cognition had been so vigorous and easily elicited a phenomenon as to make itself clearly visible in a few score trials of this kind, it would have been long ago recognized as an established fact of science. As things are, it seems clear that anyone wishing to satisfy himself *de novo* of the reality of the phenomenon must be prepared to undertake experiments on approximately the considerable, though by no means prohibitive, scale just indicated; or, alternatively, he must ally himself with others so that their combined contributions make up an equal bulk of material.

But it is also clear, which is more important from the present point of view, that, if he does this and conducts his experiments with reasonable care and intelligence, assessing his data conscientiously with the aid of the Catalogue, the technique is competent to provide him with the significant evidence he requires.

## Part IV

### Practical Points: Conclusion

#### 44

I have an uneasy feeling that one of the effects of this paper, which is necessarily loaded with a mass of somewhat tiresome detail, may be to give the reader the impression that experimentation on these lines is inordinately difficult and complicated. This would be altogether erroneous, and I think the best way to counteract or guard against it will be to run over briefly the various points which naturally arise in connection with any experiment of this type, so that the prospective experimenter may realize how very little there is to be afraid of.

1. *Choice of Percipients*: Some percipients are, as we might expect, better than others, though I do not yet know how much better, or how consistently. But beyond a rather faint indication to the effect that women of, say, 50 years of age and upwards are somewhat superior to other classes, I can offer no criterion for selecting good percipients in advance. Care should be taken to avoid persons who are too lazy to read simple instructions, too stupid to understand them, or too irresponsible to carry them out properly. Over-earnest enthusiasts likely to force their own imaginings upon the experiment are unlikely to yield good results.

2. *Type of Procedure*: There is some evidence, chiefly from Expt. II, to the effect that 'group' or 'class' experiments, in which all percipients are present together, are more likely to give good results than other types; but the evidence is slender, and this type of experiment involves either invigilation by someone other than the

experimenter or the use of percipients who can be relied upon not to talk to each other, or mutter to themselves, or look at each others' work.

All things considered, probably the 'C' type, in which the experimenter produces his originals at set times while the percipients make their attempts in their own homes, is likely to prove the most convenient in most cases.

Whatever procedure be adopted, steps must be taken absolutely to preclude all possibility of percipients' gaining knowledge of the originals by sensory means or by rational inference; but this is easily ensured, notably by always having experimenter and percipients in different rooms during the relevant periods. The need for preventing percipients being influenced, wittingly or otherwise, by each others' work has been stressed; the signing of a declaration by the percipient that he has not wittingly been so influenced might well be made standard practice.

As regards size of experiment, I am inclined to suspect that the optimum number of percipients is in the neighborhood of 20, and that performance may tend to fall off if this number be greatly exceeded. There is, of course, no *a priori* reason why we should not work with any number of percipients from one upwards, provided a sufficiency of drawings is collected, but I am sure that it is preferable to ensure this by doing several experiments rather than by using a great many percipients in a single experiment, while with only one or two percipients it is largely a matter of luck whether they happen to be good or not, and day-to-day fluctuations of ability are likely to be serious. So far as I can judge, the ideal would be a set of five experiments using fifty different originals in all and 20 different percipients in each.

3. *Choice of Originals*: A good original is the kind of thing one finds in a child's painting book—a simple outline drawing of a single, simple, unambiguous and reasonably familiar concrete object. The object represented should be such that no ordinary person could well mistake it for anything else if encountered in the flesh, so to say; and although I do not myself believe that the marks made on the paper by the experimenter have much to do with what happens, except insofar as they force him to think about the object depicted and so to associate it with the experiment in his mind, the drawing should preferably conform to the same standard—i.e., should not be liable to misinterpretation.

Although I may well have made errors of judgment here and there, I believe that most of the objects of which the names are set in Small Roman type in the Catalogue are suitable in this sense as originals, and that most of those set in *italic* type are not. I accordingly strongly suggest that, in the early stages of research at any rate, originals be selected exclusively from the ROMAN type items.

It is necessary that originals should be randomly selected in the sense (a) that there must be no risk of their being determined by events of topical interest, and (b) that there must be no scope for

rational inference by the percipients as to whether one sort of object is more likely to be chosen than another—as there might be, for example, if the experimenter were to take ten objects beginning with A for the first experiment, ten beginning with B for the second, with C for the third, etc. For casual experiments, any arbitrary system of selection will usually be good enough—such as inserting ten slips of paper randomly in the Catalogue and taking, say, the ninth item from the top of each right-hand page (or bottom of each left-hand page, etc.); but for serious and continued work I should recommend that the experimenter write each ROMAN type name on a separate small card, keep all these shuffled in a bag, and draw out ten at random and without looking at the others when an experiment is to be done. The labor involved is not serious and will save much time and trouble in the long run, as will also the entering of the appropriate values of  $c$  and  $N$  on the card so as to save reference to the Catalogue.

In doing this I think it would be better not to make out a separate card for every subheading, even if the subheadings are not italicized. That is to say, I should use a single card for BIRD, CHAIR, FLOWER, HUMAN BEING, TABLE, TREE, etc., and select the particular variety to be illustrated, if one of these is drawn from the bag, by any convenient method. Otherwise originals are likely to include an unduly high proportion of such objects as these.

So far as I can see, it is nearly always legitimate to reject an original, if there is any good reason for doing so, though naturally this could be carried too far; and I think this should be done if two or more originals randomly selected happen to resemble each other unduly closely—e.g., I would reject TIGER if CAT had already been selected, and so forth.

Naturally, percipients should not be allowed to study the Catalogue, or examine the cards; otherwise they might tend to concentrate on Roman type items and avoid italicized, and this would upset the whole arithmetical basis of the method.

4. *Instructions to Percipients*: Apart from inculcating, so far as possible, an attitude of quiet confidence and receptivity, the great thing here—and experience shows that it is far from easy—is to prevent percipients from putting down what they know must be wrong. I myself have yet to find words clear or strong enough to induce them all to abstain from drawing or describing scenes, sentiments, historical incidents, moral qualities, etc., etc.; but it should be impressed upon them that they are not to indulge their propensities in these directions, or to draw geometrical diagrams or patterns, and that the original will represent a single, simple, concrete, object. Routine instructions concerning the giving of age, sex, date, etc., may be left to the individual experimenter; but he should warn those who take part in more than one self-contained experiment that the originals used, being randomly selected, may be the same in some cases as those used in former experiments—and that the cards used in any experiment should be returned to the bag, and well shuffled

into it, after each experiment is concluded; or, more generally, that an original should not be rejected because it has been used in an earlier experiment, though the same original should not be used twice in the different parts (if there be such) of a single experiment.

I think it is definitely valuable to make percipients write down explicitly what their drawings are intended to represent.

5. *Counting Hits*: This is the only part of the technique in which real difficulties are likely to be encountered; but even here they are unlikely to be so formidable in practice as my discussion may have made them appear. In nine cases out of ten, I suppose, and probably a good deal more often than this, there will be no doubt at all as to whether a hit should be counted or not, and in the few remaining cases common sense and caution should provide an answer in the light of what has been said above.

To summarize the whole matter: Logically, we are entitled to test for the non-chance occurrence of drawings having any sort of relationship whatsoever to the original; but common sense suggests that some relation of similarity is likely to prove the most profitable, and my experience strongly indicates that similarity of idea rather than of spatial form is of predominant importance. But whatever relationship we decide on, two things are imperative; first, that whatever objects represented in percipients' drawings are counted as hits in the determination of  $h$  must also be counted from the Catalogue in the determination of  $c$ ; second, that we must not make up our minds that we are going to count some particular sort of object *because* we think it would lead to a higher score to do so, and this means, in general, that we must not decide on what to count after we have seen the data. It is perfectly legitimate to say *before* we see the data "We will count W or X or Y or Z as a hit on X"—e.g., donkey, horse, mule or zebra as a hit on HORSE—but we must not decide to count donkeys after seeing the data, just because a large number of donkeys has been drawn.

In passing, I may say that, so far as my experience goes, 'transformations' of this kind seldom occur; such transformations as do occur seem rather to be due to misinterpretation of a cognized shape (or rather, in my view, 'the idea of the shape'), as in hour-glasses for BOW, milk-bottles for THERMOS, amphorae for JUG, bunch of balloons for GRAPES, etc. If this be correct, it follows that to extend our range of acceptance in the kind of way just indicated will seldom if ever be profitable, but usually the reverse, and I accordingly strongly advise against it.<sup>1</sup>

The only completely safe course is to adhere closely to the principle of literalism (of content, not form) which has been already discussed, even though it will often involve discarding drawings which common sense vehemently assures us have very strong claims to

<sup>1</sup> It should be remembered, moreover, that, if we decide in advance to count W, X, Y or Z, etc., as hits on X, we must count the frequencies of W, Y, Z, etc., from the Catalogue in determining  $c$  even if none of these appears among the drawings.

acceptance, and I would go so far as to say that this course *must* be adopted for test purposes. In allowing ourselves a reasonable latitude when we are interested rather in information than in test scores, common sense will usually be a safe enough guide, though admittedly questions of real difficulty may occasionally arise. For example, should a drawing of a Lens System be accepted as a hit on TELESCOPE? If so, would we not also have accepted it as a hit on MICROSCOPE, or CAMERA; and should we not, therefore, count the frequencies of these objects in finding the value of  $c$ ? I think there can be little doubt that the answers to all three questions should be in the affirmative; but it is the kind of point which might easily escape the attention of the experimenter.<sup>1</sup>

Difficulties of this kind are, however, extremely rare, and to say that they may occur is to say no more than that the technique shares with almost every other scientific method the defect of not being entirely automatic, but of occasionally presenting to those who seek accuracy cases in which the experimenter's judgment must be exercised.

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In conclusion: There is no better way of spoiling a useful contribution to knowledge or to method than by claiming too much for it.

I am confident that the methods here described constitute, with the Catalogue, a good and valuable technique, in the sense that, if they are applied in the manner indicated, they will enable any competent experimenter to verify for himself the occurrence of paranormal cognition, without excessive labor, and, if he so wishes, to continue his research into the study of the kind of process that it is and of how it operates. But I should be sorry to suggest either that my exposition has been perfect or that the technique, whether in outline or in detail, is final and cannot be improved upon.

On the contrary, I have little doubt that the reader will detect many faults, of omission and commission alike, in my exegesis—which I shall be very glad to remedy if opportunity offers—while I have every hope that we shall soon find new methods which will make the present look as cumbersome and as antiquated as every early instrument looks at a later stage.

The use of drawings has at least two advantages over that of cards, which constitute virtually the only alternative at the present time. In the first place, it is more interesting—or so, at least, I have found it—both to the experimenter and to the percipients; and this means that it is much easier to induce ordinary people to work and to continue working at the subject. In the second, the very large range of possible originals invests each with, so to say, an indi-

<sup>1</sup> The Cross-references and Notes given after the Catalogue should be helpful in this kind of situation, but it is virtually impossible to make them so exhaustive as to cover all possible contingencies, and the serious experimenter should accordingly be at pains to study the Catalogue carefully and to familiarize himself thoroughly with it. The Notes should always be consulted.

viduality of its own, which is almost wholly lacking in cards and especially in the five-symbol Zener cards now commonly used. The point of this is that it enables us to connect, as it were, a drawing of a hand, say, made on occasion A with an original of a HAND displayed on occasion B, and to inquire, by the study of many such cases, whether there is any non-chance relation between the two events; whereas, with cards, the individual object too soon gets lost, so to put it, in the crowd—for who shall say whether a guess of a circle is to be regarded as possibly retrocognitive on the last circle among the target cards or as possibly precognitive on the next?

I do not, of course, pretend that the phenomenon of displacement, which I venture to regard as a key fact in the subject, would never have been made manifest with the aid of cards alone; but there can be no doubt that its discovery was greatly facilitated by this property of drawings.

Another merit arising out of this property is that it enables us to study 'cross-influences' between groups, as in Expt. VII where different groups scored freely on each others' originals, or within groups, where recent work has shown strong evidence of 'lateral telepathy' or some analogous effect, and possibly of the formation of a kind of 'group mind.'

Moreover, the fact that a drawing has both form and content, (or 'shape' and 'idea'), and that different objects and drawings thereof may have varying relations of similarity with each other, both invites and makes possible enquiries into the nature of the process involved which would be out of the question with test material lacking these properties.

On the other hand, these last mentioned properties inevitably introduce, on occasion, a certain measure of ambiguity; this can only be completely eliminated by adopting a policy of literalism which may sometimes seriously impair the sensitivity of the technique.

What is wanted is clearly a technique based on a type of test material which, while preserving some degree of human or dramatic interest, shall provide a range of possible and different test-objects comparable with that of drawings (say, not less than 500), with complete lack of ambiguity, a theoretical expectation and variance, and the possibility of scoring easy successes as well as difficult. Various possibilities are under consideration, but suggestions would be welcomed, for the problem of satisfying all these conditions is far from a simple one.

Meanwhile, the technique with drawings discussed above, provided it be not falsely regarded as more than it is, appears to constitute if not a perfect at least an entirely practical instrument of research; and I earnestly hope that, in view of the great importance of these phenomena and their implications, there will not be lacking experimenters, and particularly professional psychologists, willing to make trial of it on an extensive scale.

*Experientia docet; doceat experientia.*

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Appendix I  
Catalogue  
of  
Frequencies  
of  
Objects Drawn or Mentioned  
in  
Experiments I to VII

## Foreword to Catalogue

The left-hand column gives the names of the objects, etc., drawn or mentioned by percipients. For the differences between names printed in different sorts of type, see sections 25 and 26.

The next column shows the number of the experiment, if any, in which a drawing of the object named was used as an original; e.g., a drawing of an **ARROW** was used as an original in Expt. V. Where the entry VI\* occurs, it means that the object appeared among the forty originals drawn for potential use in Expt. VI, but not actually used. Cf. my second paper (2) p. 280. The entry VII\* indicates that the name of the object was written on one of the 100 slips sent to experimenters of Expt. VII, but not used by them. Cf. (2), p. 296. Recent work shows that the percipients of VII scored significantly above chance on these objects; and experimenters should therefore treat them as if they had been actually used as originals. The same seems to be true to a lesser extent of the objects marked VI\*, so that the same precaution should be observed. That is to say, N should be taken as 495 for objects marked VI\* and 496 for those marked VII\* and the number of entries under VI and VII respectively should be deducted from the totals in determining the value of *c*. The various sections of Expt. VII are distinguished by the appropriate letters (*Ibid.* p. 294).

The next pair of columns show the number of percipients who drew or mentioned the object concerned in the course of the first five experiments, either as the sole or virtually sole content of a drawing ( $\alpha$ ) or in conjunction with another object or objects ( $\beta$ ). Cf. section 24. Similar data for Expts. VI and VII are given in the pairs of columns indicated by these figures.

The last three columns give the sum of the  $\alpha$  entries, the sum of the  $\beta$  entries, and the sum of all entries in the line, in order.

Cross-references and Notes, which I fear I cannot claim to be exhaustive, are given immediately after the Catalogue. Experimenters should make a practice of *always* referring to these when using the Catalogue.

# ABBREVIATIONS.

The following abbreviations are used:

~	= Approximately, more or less, substantially, etc.
Carprs.	= Carpenter's.
Cheml.	= Chemical.
Convl.	= Conventional.
D or Drg.	= Drawing.
Geoml.	= Geometrical.
Incl.	= Includes; or Including.
Illd.	= Illustrated.
Indt.	= Indeterminate.
Mechl.	= Mechanical.
n.o.l.	= Not otherwise listed.
n.s.l.	= Not separately listed.
O or Orgl.	= Original.
Ord.	= Ordinary.
Ornl.	= Ornamental.
o.t.	= Other than.
pr.	= Presumable, presumably, etc.
Sp. Mn.	= Specifically mentioned.
Unspd.	= Unspecified.
Var.	= Various.

Catalogue of Frequencies  
of  
Objects Drawn or Mentioned  
in  
Experiments I to VII

Name of OBJECT	Orgl. in Expt.	I-V		VI		VII		TOTALS		
		$\alpha$	$\beta$	$\alpha$	$\beta$	$\alpha$	$\beta$	$\alpha$	$\beta$	Total
ABACUS OF BALL-FRAME	VII*	1	.	.	.	.	.	1	.	1
Ace of Clubs	VIIc	.	.	.	.	.	.	.	.	.
ACORN	VII*	.	.	.	.	1	.	1	.	1
AEROPLANE	VII*	10	1	16	3	7	4	33	8	41
AIRSCREW	VII*	3	.	.	.	.	.	3	.	3
AIRSHIP (Dirigible)	.	.	.	1	.	1	.	2	.	2
Alidade	.	.	.	1	.	.	.	1	.	1
ALLIGATOR, See Crocodile	.	.	.	.	.	.	.	.	.	.
Altar (Christian)	.	.	.	1	1	.	1	1	2	3
Ancestor	II	.	.	1	.	.	.	1	*	1
ANCHOR	I	12	1	4	1	1	2	17	4	21
Andirons	.	.	.	1	.	.	.	.	1	1
ANGEL	VI*	1	1	2	2	2	.	5	3	8
Animal, Indt. or Unspd.	.	11	3	1	.	1	1	13	4	17
ANTELOPE OF ~	.	.	.	.	.	2	.	2	.	2
ANVIL	.	.	.	1	1	1	.	2	1	3
APPLE	.	19	9	24	14	19	20	62	43	105
ARCH OF ARCHWAY, Ord.	.	4	3	4	1	3	4	11	8	19
" (Arc de Triomphe)	.	.	.	2	.	.	.	2	.	2
ARK, Noah's	.	.	.	2	.	1	.	3	.	3
Arm, Sp. Mn., etc.	.	1	1	.	1	.	1	1	3	4
Armour	.	.	.	.	1	.	.	.	1	1
Army. See Troops (HUMAN BEINGS)	.	.	.	.	.	.	.	.	.	.
ARROW, Full	V	7	4	4	9	1	1	12	14	26
" Partial	.	3	3	2	1	1	.	6	4	10
Ashcan, See Dustbin	.	.	.	.	.	.	.	.	.	.
ASHTRAY	.	.	2	.	.	.	2	.	4	4
AUTOMOBILE, See MOTOR CAR	.	.	.	.	.	.	.	.	.	.
Avenue, See Tree	.	.	.	.	.	.	.	.	.	.
Awl	.	.	.	1	.	.	.	1	.	1
AXE, ~ Battle	.	.	.	1	.	1	.	2	.	2
" ~ Executioner's	.	2	1	1	.	.	.	3	1	4
" ~ Woodman's	.	.	.	.	.	.	1	.	1	1
Baby, See HUMAN BEINGS	.	.	.	.	.	.	.	.	.	.
BAG, Hand	VI*	1	.	5	.	1	2	7	2	9
" Net	.	1	1	.	.	.	1	.	2	2
" Travelling	.	1	.	.	.	.	.	1	.	1
BALANCE OF SCALES, Ord.	IV	7	.	1	.	3	.	11	.	11
" Spring	.	.	.	.	.	1	.	1	.	1
BALL, Indt., or var.	.	5	6	4	3	7	12	16	21	37
" Cricket	.	1	1	.	.	.	2	1	3	4
" Football, Assn.	.	.	.	1	.	3	1	4	1	5
" Rugby	.	2	1	2	1	1	.	5	2	7
" Snow	.	.	.	.	2	.	.	.	2	2
" Tennis	.	.	3	.	1	1	4	1	8	9
BALL OF WOOL	.	.	.	.	1	.	3	.	4	4
Balls, colored, heap of	.	.	.	1	.	.	.	1	.	1

	Expt. Orgl. in	$\alpha$ I-V	$\beta$	$\alpha$ VI	$\beta$	$\alpha$ VII	$\beta$	$\alpha$ TOTALS	$\beta$	Total
BALLOON, Free, Gondola of -	.	.	.	1	.	.	.	1	.	1
" Kite or Barrage -	.	1	.	2	.	6	.	9	.	9
" Child's -	.	.	.	.	.	1	1	1	1	2
BALLOONS, Child's, Bunch of -	.	.	.	2	3	.	.	2	3	5
Balustrade -	.	2	1	.	1	.	.	2	2	4
Bamboos or ~ -	.	1	1	.	.	.	.	1	1	2
BANANA -	VI*	2	2	.	.	12	14	19	23	42
Bandstand -	.	.	.	1	.	.	.	1	.	1
Banisters, Sp. Mn. -	.	.	.	.	.	1	.	1	.	1
BANJO -	.	1	.	2	.	.	.	3	.	3
Barge, Stern only -	.	.	.	1	.	.	.	1	.	1
BAROMETER -	VII*	.	.	.	.	.	.	.	.	.
BARREL or CASK -	VI*	2	.	6	.	3	2	11	2	13
" Water built -	.	.	1	.	2	.	.	.	3	3
BARROW, ~ Coster's -	.	3	.	.	1	1	.	4	1	5
" ~ Porter's -	.	1	1	.	.	1	2	2	3	5
BASIN, See BOWL										
BASKET, Empty or unspd. -	VII*	4	4	8	1	5	5	17	10	27
" with contents -	.	.	2	.	5	.	7	.	14	14
BAT (Animal) -	I	1	.	1	.	.	.	2	.	2
BAT (Baseball) -	.	1	.	.	.	.	.	1	.	1
" (Cricket) -	VII*	1	2	1	1	7	1	9	4	13
BATH, Ord. -	VI*	.	.	1	.	1	.	1	1	2
" Foot -	.	.	.	.	.	1	.	1	.	1
Battledore -	.	.	1	.	.	.	.	.	1	1
Battlements -	I	.	.	.	.	2	.	2	.	2
Bauble, Jester's -	.	1	.	.	.	.	.	1	.	1
BAYONET -	.	.	.	1	.	.	.	1	.	1
Beach, Sp. Mn. -	.	.	1	.	3	1	.	5	.	5
Beacon, Belisha -	.	.	1	.	.	3	1	3	2	5
BEAR, Ord. or unspd. -	VI*	1	.	.	2	.	.	1	2	3
" 'Teddy' -	.	.	1	4	.	1	.	5	1	6
BED, Ord. -	VII*	2	.	2	1	3	1	7	2	9
BEE -	.	1	1	1	.	.	.	2	1	3
BEEHIVE -	VII*	1	1	.	.	.	.	1	1	2
BETLE -	I	.	.	1	.	.	.	1	.	1
BELL, ~ Church -	VI*	12	.	16	.	4	1	32	1	33
" Hand -	.	1	.	5	.	2	.	8	.	8
" Desk -	.	1	.	.	.	.	.	1	.	1
Bell-Push -	.	.	.	.	.	1	.	1	.	1
BELLOWS -	VII*	.	.	.	.	1	.	1	.	1
BELT, Mechl. -	.	.	1	.	.	.	.	.	1	1
" Trousers -	VII*	.	.	.	.	1	.	1	.	1
BENCH, ~ Garden Seat -	V	.	.	2	2	3	.	2	5	7
BICYCLE -	VI*	6	.	6	4	4	5	16	9	25
BINOCULARS -	VII*	.	.	.	.	1	.	1	.	1
BIRD, single, ord. or unspd.,										
" not flying -	III	14	3	8	4	6	3	28	10	38
" as above, flying	.	5	.	4	1	.	.	9	1	10
plural, ord. or unspd.,										
flying or unspd. -	.	2	2	1	1	2	2	5	5	10
" fantastic, toy -	.	1	.	.	.	.	.	1	.	1
" young, in nest -	.	.	.	.	.	1	.	.	1	1
" Chick -	.	.	1	1	1	.	.	2	2	4
" Cock -	VIIa	2	1	4	.	5	.	11	1	12
" Cockatoo -	.	.	.	1	1	.	.	1	1	2
" Crow -	.	.	.	1	.	1	.	2	.	2
" Dodo -	IV	.	.	.	.	.	.	.	.	.
" Duck -	.	3	.	8	1	4	2	15	3	18

Name of OBJECT		Orgl. in Expt.	I-V		VI		VII		TOTALS		
			$\alpha$	$\beta$	$\alpha$	$\beta$	$\alpha$	$\beta$	$\alpha$	$\beta$	Total
BIRD,	Eagle - - - -	VII*	2	1	.	.	1	.	3	1	4
"	Goose - - - -	.	2	2	2	.	2	.	6	2	8
"	Gull - - - -	.	1	1	2	1	1	1	4	3	7
"	Hen - - - -	.	2	2	1	.	2	1	5	3	8
"	Kiwi - - - -	.	.	.	1	.	.	.	1	.	1
"	Ostrich - - - -	.	1	.	1	.	3	.	5	.	5
"	Owl - - - -	.	5	2	10	.	4	3	19	5	24
"	Parrakeet - - - -	.	.	.	1	.	.	.	1	.	1
"	Parrot - - - -	.	1	.	1	.	.	.	2	.	2
"	Peacock - - - -	VII*	2	.	4	.	1	.	7	.	7
"	Pelican - - - -	VI	1	.	1	.	.	.	2	.	2
"	Penguin - - - -	.	.	.	.	.	1	.	1	.	1
"	Pigeon or Dove - - - -	.	.	.	1	.	1	2	2	2	4
"	Poultry, unspd. - - - -	.	.	1	.	.	.	.	1	.	1
"	Robin - - - -	.	.	1	.	.	1	.	1	1	2
"	Rook - - - -	.	.	.	.	.	1	.	1	1	1
"	Swallow or Swift - - - -	VII*	.	.	.	.	2	1	2	1	3
"	Swan - - - -	.	5	1	11	4	5	5	21	10	31
"	Toucan - - - -	.	.	.	.	.	1	.	1	.	1
"	Turkey - - - -	.	.	.	1	.	.	.	1	.	1
"	Vulture - - - -	.	.	.	2	.	.	.	2	.	2
BIRDCAGE, See CAGE											
Biscuit - - - -	.	.	1	.	.	1	.	.	1	1	2
Blackboard - - - -	.	.	.	1	.	5	.	5	.	11	11
Block, Chopping - - - -	.	.	.	1	.	.	.	.	.	1	1
Blocks, Child's toy - - - -	.	.	1	.	1	.	.	.	2	.	2
Blotter, Curved, hand - - - -	.	.	.	.	1	.	.	.	1	.	1
" Flat, desk - - - -	.	.	.	.	.	2	.	1	.	3	3
BOAT, Slg., F & A, 1 mast - - - -	V	26	4	36	5	20	11	82	20	102	
" Slg., F & and 2 masts - - - -	.	4	1	4	1	2	1	10	3	13	
" Slg., Sq. rig, 1 mast - - - -	.	2	1	2	1	2	.	6	2	8	
" Mast but no Sail - - - -	.	2	.	.	.	.	1	2	1	3	
" Sail only - - - -	.	1	1	1	.	.	.	2	1	3	
" Slg., small, pl. - - - -	.	1	.	.	.	.	.	1	.	1	
" Motor or Speed - - - -	.	.	1	.	1	1	.	1	2	3	
" Rowing - - - -	.	.	4	4	3	5	1	9	8	17	
" " Racing eight - - - -	.	.	1	.	.	.	1	.	2	.	2
" Small, open, indt. - - - -	.	2	1	.	.	2	.	4	1	5	
" Indt., forepart only - - - -	.	1	.	1	.	.	.	2	.	2	
Bobbin (not Reel) - - - -	.	.	1	.	.	.	.	1	.	1	
Boiler, Steam - - - -	.	.	.	.	1	.	.	.	1	1	1
" Tar - - - -	.	.	1	.	.	.	.	.	1	1	1
Bollard - - - -	.	.	.	.	1	.	.	.	1	1	1
BOMB - - - -	.	.	.	2	1	.	2	.	2	3	5
Bone(s) o.t. skeleton - - - -	.	1	.	2	.	.	.	3	.	3	3
BONFIRE - - - -	VII*	.	1	1	.	2	.	3	1	4	4
BOOK, single or ~ - - - -	VI	42	6	38	11	37	10	117	27	144	
" row or pile - - - -	.	2	1	6	2	3	7	11	10	21	21
BOOKCASE - - - -	.	.	.	.	1	.	3	.	4	.	4
Book-ends - - - -	.	.	.	.	1	.	3	.	1	3	4
Bookmarker - - - -	.	.	.	.	1	.	.	.	1	.	1
BOOT(s), Ord. or Riding - - - -	IV	3	1	6	.	6	.	15	1	16	16
" Gum - - - -	.	1	.	2	.	.	.	3	.	3	3
BOTTLE, Var. & unspd. - - - -	.	2	1	4	2	4	3	10	6	16	16
" Milk or ~ - - - -	.	.	1	1	2	4	3	5	6	11	11
" Wine or ~ - - - -	I	11	1	6	4	3	3	20	8	28	28
" Rubber, Hot-water - - - -	.	2	.	.	.	1	.	3	.	3	3
Boulder, Sp. Mn. - - - -	.	.	1	.	.	.	.	.	1	.	1



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Name of OBJECT	Orgl. in Expt.	I-V		VI		VII		TOTALS		
		$\alpha$	$\beta$	$\alpha$	$\beta$	$\alpha$	$\beta$	$\alpha$	$\beta$	Total
Bow, Archery - - - -	.	.	4	.	3	.	.	.	7	7
" <i>Crossbow</i> - - - -	.	.	.	.	.	1	.	1	.	1
Bow, Ribbon or Tie - -	VI	.	.	3	1	2	2	5	3	8
Bow, Violin - - - -	.	.	.	.	.	2	.	.	2	2
Bowl, Shallow, Empty -	.	1	.	.	1	.	1	1	2	3
" " not Empty - - -	.	.	2	.	4	.	5	.	11	11
" $\frac{1}{2}$ -sphl., Empty - -	VIIe	4	.	7	3	3	5	14	8	22
" " not Empty - - -	.	.	6	.	8	.	8	.	22	22
" $\frac{3}{4}$ -sphl., Empty - -	.	1	.	1	.	1	.	3	.	3
" " not Empty - - -	.	.	3	.	4	.	.	.	7	7
" " with goldfish - -	.	.	1	.	1	.	5	.	7	7
" Sugar - - - - -	.	1	.	.	.	.	.	1	.	1
Box, <i>Cylindrical</i> - - -	.	.	2	2	.	1	1	3	3	6
" Rectlr., Closed - - -	.	4	4	16	3	11	1	31	8	39
" Lid open - - - - -	VIIb	7	.	3	.	6	3	16	3	19
" Gas mask - - - - -	.	.	.	.	.	1	1	1	1	2
" Match, Ord., sliding -	.	4	1	1	1	5	4	10	6	16
" Musical, See Musical										
Boy, See HUMAN BEINGS										
BRACES (Trousers) - - -	VIIa	.	1	.	.	.	1	.	2	2
Bracket, Orn., Iron - -	.	1	1	.	.	.	1	1	2	3
" Plain Wood - - - -	I	2	.	1	.	.	.	3	.	3
Branch of Tree - - - -	.	3	.	.	2	.	5	3	7	10
BRAZIER - - - - -	.	.	1	.	.	1	.	1	1	2
BREAD, See LOAF										
BRICKS, Rows of, not wall	.	2	.	.	.	.	.	2	.	2
BRIDGE, Arched - - - -	VI*	11	11	8	3	7	7	26	21	47
" Bascule - - - - -	.	.	.	2	.	.	.	2	.	2
" Girder - - - - -	.	.	.	.	.	1	.	.	1	1
" Suspension - - - - -	VII*	.	.	2	.	.	1	2	1	3
Brim - - - - -	II	.	.	1	.	.	.	1	.	1
BROOM, T-head or unspd. -	VII*	2	1	3	.	2	2	7	3	10
" Besom - - - - -	.	1	.	1	.	2	.	2	2	4
BRUSH, Artist's - - - -	.	.	1	.	.	2	.	.	3	3
" Clothes or scrub - -	.	1	.	1	.	1	2	3	2	5
" Hair, with handle - -	.	.	.	1	.	1	2	2	2	4
" Housepainter's - - -	.	3	1	.	.	.	.	3	1	4
" Paste - - - - -	.	.	.	1	.	.	.	.	1	1
" Shaving - - - - -	.	.	1	.	.	1	.	1	1	2
" Tooth - - - - -	VIIa	.	.	2	.	6	.	8	.	8
" Whitewash - - - - -	.	.	.	.	.	1	.	1	.	1
Bubble - - - - -	.	.	.	.	.	1	.	1	.	1
BUCKET or PAIL - - - -	VII*	7	1	8	7	11	11	26	19	45
BUCKLE, Shoe - - - - -	.	.	.	.	.	1	.	.	1	1
" Belt - - - - -	.	.	.	.	.	1	.	.	1	1
Buds of leaves, in vase -	.	.	1	.	.	.	.	.	1	1
BUFFALO - - - - -	I	.	.	.	.	.	.	.	.	.
Buffalo Horns - - - -	.	1	.	.	.	.	.	1	.	1
BUGLE - - - - -	.	2	.	.	.	1	.	3	.	3
Building, Indt. or var., n.q.L.	.	4	2	11	8	2	6	17	16	33
BULB, Electric, not in fitting	VII*	1	1	2	.	2	.	5	1	6
" Geissler Potash - - -	V	.	.	.	.	.	.	.	.	.
" Glass, with tube - - -	.	2	.	1	.	.	.	3	.	3
" " no tube - - - - -	.	1	.	.	.	.	.	1	.	1
" Horticultural - - - -	.	.	.	1	.	.	.	1	.	1
BULL, See COW										
Bullet - - - - -	.	.	.	1	.	.	.	1	.	1
Bunsen Burner - - - - -	.	.	.	.	.	1	.	1	.	1

Name of OBJECT	Orgl. in Expt.	I-V		VI		VII		TOTALS		
		$\alpha$	$\beta$	$\alpha$	$\beta$	$\alpha$	$\beta$	$\alpha$	$\beta$	Total
BUS, Horse - - - - -	.	.	.	1	.	.	.	1	1	1
" Motor - - - - -	.	1	.	1	.	.	.	1	1	2
Bush or Shrub - - - - -	.	.	2	2	.	2	.	6	.	6
BUTTERFLY or MOTH - - - - -	IV	7	.	13	1	10	1	30	2	32
BUTTON - - - - -	.	2	.	1	.	1	.	4	.	4
CABBAGE - - - - -	VI*	.	.	2	.	2	1	4	1	5
CAGE, Bird - - - - -	VII*	.	.	4	1	1	.	5	1	6
CAKE, Christmas - - - - -	.	.	.	.	.	1	.	1	.	1
" small, pl. - - - - -	.	.	1	.	.	1	.	.	2	2
Cake-stand - - - - -	.	.	1	.	.	.	.	.	1	1
CALENDAR - - - - -	VII*	1	.	1	.	.	.	2	.	2
CAMEL - - - - -	VI*	2	.	4	2	4	1	10	3	13
CAMERA, Box - - - - -	VIIb	.	.	.	.	2	.	2	.	2
" Folding - - - - -	.	.	.	1	.	2	.	3	.	3
Camera Case & Sling - - - - -	.	.	.	1	.	.	.	1	.	1
Can, Milk, enamel - - - - -	.	.	.	1	.	.	.	1	.	1
Canal - - - - -	.	.	.	.	.	1	.	1	.	1
CANDLE, No candlestick - - - - -	.	4	2	3	.	3	2	10	4	14
" In candlestick - - - - -	.	.	12	.	18	.	15	.	45	45
CANDLESTICK, Bedroom - - - - -	.	.	8	.	12	.	11	.	31	31
" Upright - - - - -	.	3	4	.	6	1	4	4	14	18
Candy, Bar of - - - - -	.	1	.	.	.	.	.	1	.	1
CANNON, See GUN, Field										
CANOE - - - - -	VI*	.	.	2	3	2	.	4	3	7
Canvas, Artist's - - - - -	.	.	1	.	.	1	.	.	2	2
CAP, Ord. or Jockey - - - - -	VI*	.	.	.	.	2	.	2	.	2
" Academic - - - - -	.	.	1	1	.	.	.	1	1	2
" Beret - - - - -	.	1	.	.	.	1	.	1	1	2
" Dunce's - - - - -	.	.	.	.	.	1	.	1	.	1
" Military - - - - -	.	.	.	3	.	.	.	3	.	3
Cape - - - - -	.	1	.	.	.	.	.	.	1	1
CAPSTAN - - - - -	VI*	1	.	.	.	1	.	2	.	2
CARAFE - - - - -	.	4	.	4	3	1	1	9	4	13
Caravan - - - - -	.	.	1	.	.	1	.	1	1	2
Card, Blank - - - - -	.	1	.	.	.	.	.	1	.	1
" Christmas - - - - -	.	.	1	.	.	.	.	.	1	1
" Playing, var. - - - - -	.	7	1	1	.	1	.	9	1	10
Carpet - - - - -	.	.	.	1	.	1	.	1	1	2
CARPET-SWEEPER - - - - -	.	1	.	.	.	1	.	2	.	2
Carpet-switch - - - - -	.	.	.	1	.	1	.	2	.	2
Carriage, Horsed - - - - -	.	.	.	.	1	.	.	.	1	1
CARROT - - - - -	VIIc	5	.	6	1	4	.	15	1	16
CART, Four wheels - - - - -	VII*	2	.	2	3	.	2	4	5	9
" Two wheels - - - - -		4	2	1	.	3	6	8	8	16
CASE, Suit or Attaché - - - - -	.	.	.	1	.	5	1	6	1	7
CASTLE, Ord. or Convl. - - - - -	IV	4	1	4	2	4	4	12	7	19
" as in chess - - - - -	.	.	.	1	.	.	.	1	.	1
CAT or KITTEN - - - - -	VII*	14	1	13	6	28	11	55	18	73
CATAPULT - - - - -	VIIc	.	.	.	.	.	.	.	.	.
" v. $\sim$ shape - - - - -	.	2	.	.	.	.	.	2	.	2
CATERPILLAR - - - - -	VIIa	1	.	.	.	.	.	1	.	1
CAULDRON - - - - -	.	1	.	.	1	.	.	1	1	2
CAVE - - - - -	.	1	.	.	.	1	1	2	1	3
Celt (Implement) - - - - -	.	1	.	.	.	.	.	1	.	1
CENOTAPH, See OBELISK										
CHAIN, Ord., Watch or unspd.	.	3	2	1	.	1	.	5	2	7
CHAIR, Ord., upright, no arms	VII*	29	6	21	8	25	12	75	26	101

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Name of OBJECT	Orgl. in Expt.	I-V		VI		VII		TOTALS		Total
		$\alpha$	$\beta$	$\alpha$	$\beta$	$\alpha$	$\beta$	$\alpha$	$\beta$	
CHAIR, Upright, with arms -	.	4	4	2	.	2	.	8	4	12
" Markedly ceremonial -	.	2	1	.	.	2	.	3	1	4
" Easy, arm -	.	4	1	4	1	6	3	14	5	19
" Deck -	.	.	.	1	1	2	1	2	2	4
" Rocking -	.	.	.	1	1	.	.	1	1	2
Cheese -	.	1	.	1	.	2	.	3	1	4
Cheese Dish, with cover -	.	.	.	.	.	1	.	1	.	1
CHERRY -	.	1	.	1	.	2	.	4	.	4
Chessboard -	.	3	1	1	.	1	.	5	1	6
Chessmen, var., unspd. -	.	.	1	.	.	.	.	.	1	1
CHEST OF DRAWERS -	VII*	2	.	1	.	1	1	4	1	5
Child, See HUMAN BEINGS										
CHIMNEY, Factory -	VIIb	2	.	1	1	1	2	4	3	7
Chimney Pot, Domestic -	.	.	1	.	1	.	1	.	3	3
CHISEL -	VI*	.	.	.	.	.	.	.	.	.
CHOP, Meat -	.	.	.	.	.	1	.	.	1	1
CHOPPER or HATCHET -	VI	1	.	5	.	.	.	6	.	6
CHURCH, ord. or unspd. -	VIIc	10	2	15	4	11	5	36	11	47
" Abbey -	.	1	.	.	.	.	.	1	.	1
" Cathedral -	.	.	.	.	.	1	.	1	1	1
" Chapel, King's Coll. -	.	.	1	1	.	.	.	1	1	2
CIGAR -	VI	.	.	2	.	2	2	4	2	6
CIGARETTE -	.	1	3	2	3	4	1	7	7	14
" Carton -	.	.	.	.	.	1	.	1	.	1
Clarinet -	.	.	1	.	.	.	.	1	1	1
Claw, Bird's or ~ -	.	.	1	1	.	.	.	1	1	2
CLEOPATRA' NEEDLE, See OBELISK										
Cliff -	VI*	.	12	3	7	1	5	4	24	28
Cloak -	.	.	1	.	.	.	.	.	1	1
CLOCK, Ord. or unspd. -	.	12	.	12	2	16	2	40	4	44
" Face only -	.	4	.	5	2	1	2	10	4	14
" Alarm -	.	.	.	4	1	4	.	8	1	9
" Big Ben -	.	.	.	1	.	.	.	1	.	1
" Grandfather -	.	1	.	7	.	2	.	10	.	10
" Pendulum wall -	.	2	.	3	.	1	.	5	1	6
CLOG, See SABOT										
Clothes, var., on line -	.	.	2	.	.	1	.	3	.	3
CLOTHES PEG -	.	1	.	.	.	2	.	3	.	3
Cloud(s), Sp. Mn. -	.	2	8	1	3	1	5	4	16	20
Club -	.	1	.	.	.	.	.	1	.	1
Coach, Horsesd -	.	.	1	.	.	.	.	.	1	1
COAL SCUTTLE, var. -	.	2	1	1	.	4	.	7	1	8
COAT or JACKET, etc. -	VI	1	1	3	3	2	.	4	6	10
COAT-HANGER -	.	.	.	1	.	1	.	2	.	2
COFFEE MILL -	.	1	.	1	.	.	.	2	.	2
Coffee Pot -	.	1	.	1	.	.	.	2	.	2
COFFIN -	VI*	1	1	.	.	1	.	2	1	3
COIN, var. -	.	.	.	2	.	5	1	7	1	8
COLLAR, Odr. -	.	.	.	.	1	2	.	2	1	3
" Quaker -	.	.	.	.	1	.	.	.	1	1
COLUMN or PILLAR -	.	2	2	3	3	1	2	6	7	13
COMB -	VIIc	1	.	.	.	2	2	3	2	5
COMET or v. ~ shape -	.	1	.	.	.	1	.	1	1	2
Cone -	.	2	1	1	.	.	.	3	1	4
Cooker, Electric -	.	.	.	1	.	.	.	1	.	1
CORK -	.	.	1	.	2	.	2	.	5	5
CORKSCREW -	VIIb	.	.	1	.	3	.	4	.	4
Corn, in field -	.	.	.	.	.	1	.	1	.	1

Experiments on the Paranormal Cognition of Drawings

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Name of OBJECT	Orgl. in Expt.	I-V		VI		VII		TOTALS		
		$\alpha$	$\beta$	$\alpha$	$\beta$	$\alpha$	$\beta$	$\alpha$	$\beta$	Total
Corpses - - - - -			1						1	1
Cotton Aphid - - - - -	III									
Cotton Boll - - - - -		1						1		1
Counterpane - - - - -				1					1	1
Cow, etc. - - - - -		5	5	3	4	9	3	17	12	29
Cowl, Nun's - - - - -				1				1		1
CRAB - - - - -		1		1		1		3		3
CRACKER (Christmas) - - - - -				1				1		1
CRADLE - - - - -		2		1	1			3	1	4
CRAMP, Carprs. - - - - -	VII*									
CRANE (Mechl.) or DERRICK - - - - -	VII*			1		2	1	2	2	4
Crankschaft - - - - -				1				1		1
Crinoline - - - - -			4		2		3		9	9
CROCODILE or ALLIGATOR - - - - -	VII*			3				3		3
Cromlech (Stonehenge) - - - - -				1				1		1
Crook, Shepherd's - - - - -				1					1	1
CROSS, Latin or Greek - - - - -	V	26	8	12	4	3	1	41	13	54
" " solid - - - - -		3		1	1	1	1	5	2	7
" St. Andrew's - - - - -		5	1	1	1			6	2	8
" Maltese - - - - -						2		2		2
" Market - - - - -						1	1	1	1	2
" 'plus-sign' - - - - -		2	4	1	1	1		4	5	9
Crossbones - - - - -		1	1				2	1	3	4
Crossbow, See Bow										
Cross-stitch - - - - -	I									
CROWN - - - - -	VIIa	8	1	9	3	4	1	21	5	26
Crucifix - - - - -				1		2		3		3
Cruet - - - - -						1			1	1
CRUTCH - - - - -	VII*									
Crystal, polyhedral - - - - -		1				1		2		2
CUCUMBER - - - - -		1		1		1		3		3
Cue, Billiard - - - - -					2		1		3	3
CUFF-LINK, See LINKS										
CUP, no Saucer - - - - -	VIIId	2		4		12	2	18	2	20
" with Saucer - - - - -			10		16		24		50	50
" Two-handled - - - - -				1		1		2		2
Currants - - - - -			1						1	1
CURTAIN - - - - -	VI*	3	4		5	12		3	21	24
CUSHION - - - - -			1		1	2		4	4	4
Cylinder, Indt., unspd. - - - - -		2			1	1		3	1	4
DAGGER - - - - -	VII*	2		2		1		5		5
DART, Ord. as for game - - - - -				1	1			1	1	2
DARTBOARD - - - - -					1				1	1
Decanter - - - - -		1			3	1	1	2	4	6
DERRICK, See CRANE										
DESK, School or Office - - - - -			1	2	3		2	2	6	8
Dessicator, Cheml. - - - - -		1						1		1
DEVIL - - - - -				2	1			2	1	3
Dew, Sp. Mn. - - - - -			1						1	1
DIE or DICE - - - - -		1		3				4		4
Dipper, Milk - - - - -					1				1	1
DISH, Shallow - - - - -			3	1	3		3	1	9	10
" ~ 1/5 deep - - - - -				2	3		2	2	5	7
" ~ 1/3 - - - - -			1		1		1		3	3
" with handle, unspd. - - - - -		1						1		1
" ornamental, " - - - - -		1						1		1
" sectored, as for hors d'oeuvres - - - - -		1				1		2		2
Ditch - - - - -		1						1		1

Name of OBJECT	Orgl. in Expt.	I-V		VI		VII		TOTALS		
		$\alpha$	$\beta$	$\alpha$	$\beta$	$\alpha$	$\beta$	$\alpha$	$\beta$	Total
<i>Dividers (Geoml. Inst.)</i> - - -	.	3	.	1	.	.	.	4	.	4
<i>Diving Board</i> - - -	.	.	.	.	.	1	.	.	1	1
<i>Dock (Dry)</i> - - -	.	.	.	.	.	1	.	.	1	1
<i>DOG (All breeds)</i> - - -	VII*	13	8	14	10	11	12	38	30	68
<i>Doll</i> - - -	.	.	1	.	2	.	2	.	5	5
<i>Doll's House</i> - - -	.	.	1	.	.	1	.	1	1	2
<i>Dome (not on building)</i> - - -	.	1	.	.	.	.	.	1	.	1
<i>DOMINO (as for game)</i> - - -	.	1	.	.	.	.	.	1	.	1
<i>DONKEY</i> - - -	.	2	2	2	1	1	.	5	3	8
<i>DOOR</i> - - -	.	7	6	10	3	4	10	21	19	40
<i>Door-knocker</i> - - -	.	1	.	.	.	.	.	1	.	1
<i>DOVE-COTE</i> - - -	.	.	.	.	.	2	.	.	2	2
<i>DRAGON</i> - - -	.	1	.	.	.	1	.	1	1	2
<i>DRAGONFLY</i> - - -	.	.	.	1	.	2	.	3	.	3
<i>Drawing Board</i> - - -	.	.	.	.	.	1	.	.	1	1
<i>Dress or Frock, Sp. Mn.</i> - - -	.	.	1	.	.	3	.	.	4	4
"    "    no wearer - - -	.	.	.	1	.	1	.	2	.	2
<i>Dresser, Kitchen</i> - - -	.	.	.	1	.	.	.	1	.	1
<i>Drill, Mechl.</i> - - -	VI*	.	.	.	.	.	.	.	.	.
<i>Drop of Water</i> - - -	.	.	.	.	.	1	.	1	.	1
<i>DRUM</i> - - -	.	.	1	2	1	1	3	3	5	8
<i>Drumsticks</i> - - -	.	.	.	1	.	3	.	.	4	4
<i>Dumbell</i> - - -	.	2	.	1	.	.	.	3	.	3
<i>Dustbin</i> - - -	.	.	.	2	.	.	.	2	.	2
<i>EAR of Corn or Wheat</i> - - -	.	1	.	3	1	.	.	4	1	5
"    Human - - -	VI*	3	.	2	.	.	.	5	.	5
<i>Earth or Soil, Sp. Mn.</i> - - -	.	1	2	.	.	.	.	1	2	3
<i>EASEL</i> - - -	VIIb	.	4	1	5	.	8	1	17	18
<i>Echinoderm, unspd.</i> - - -	.	.	.	.	.	1	.	1	.	1
<i>Ectoplasm, Cloud of</i> - - -	.	1	.	.	.	.	.	1	.	1
<i>EGG, Ord.</i> - - -	VII*	6	6	2	6	.	9	8	21	29
" <i>Easter</i> - - -	.	.	.	1	.	1	.	2	.	2
" <i>Fried</i> - - -	.	.	1	.	2	.	1	.	4	4
<i>EGG-CUP</i> - - -	VI*	1	3	2	3	1	8	4	14	18
<i>EGG-TIMER, See HOUR-GLASS</i> - - -	.	.	.	.	.	.	.	.	.	.
<i>ELEPHANT</i> - - -	VIIId	1	.	5	.	12	2	18	2	20
<i>Embroidery, Mn.</i> - - -	.	.	1	.	.	.	.	.	1	1
<i>ENGINE, Railway, See Loco-</i> <i>MOTIVE</i>	.	.	.	.	.	.	.	.	.	.
<i>Engine, Oil or Steam, Stat'ny</i> - - -	.	1	.	.	1	.	.	1	1	2
" <i>Traction</i> - - -	.	1	.	1	.	.	.	2	.	2
<i>ENVELOPE</i> - - -	VII*	6	.	8	.	5	.	19	.	19
<i>Eraser (Rubber)</i> - - -	.	.	.	1	.	.	.	1	.	1
<i>EWER, See JUG</i> - - -	.	.	.	.	.	.	.	.	.	.
<i>Exfoliate, See LEAF</i> - - -	II	.	.	.	.	.	.	.	.	.
<i>EYE, Human</i> - - -	.	5	.	3	2	5	1	13	3	16
<i>FAN, Ord.</i> - - -	IV	3	.	8	1	5	.	16	1	17
" <i>Electric</i> - - -	.	2	.	.	.	.	.	2	.	2
<i>Farm or Farmyard, Sp. Mn.</i> - - -	.	1	2	1	1	.	1	2	4	6
<i>Faun</i> - - -	.	.	2	.	.	.	.	.	2	2
<i>FEATHER, ord.</i> - - -	VII*	7	.	7	2	3	.	17	2	19
" <i>'Plume'</i> - - -	.	.	1	1	.	.	1	1	2	3
<i>Feather Duster</i> - - -	.	.	.	.	.	1	.	1	.	1
<i>Fenland, Sp. Mn.</i> - - -	.	.	1	.	.	.	.	.	1	1

Name of OBJECT	Orgl. in Expt.	I-V		VI		VII		TOTALS		
		$\alpha$	$\beta$	$\alpha$	$\beta$	$\alpha$	$\beta$	$\alpha$	$\beta$	Total
Fence or Railings, mainly horl.	.	.	8	2	5	1	2	3	15	18
" " " " vertl.	.	1	3	.	2	4	6	5	11	16
" other types or unspd.	.	.	1	.	2	.	3	.	6	6
Fender (Fire) - - - -	.	.	.	.	.	1	.	1	.	1
FERN or - - - -	.	1	.	2	.	1	.	4	.	4
Fess, Embattled - - -	I	.	.	.	.	.	.	.	.	.
Field or Meadow, Sp. Mn.	.	1	3	14	.	12	.	1	29	30
FIG - - - -	.	1	1	.	.	.	.	1	1	2
File, Lever arch - - -	.	1	.	.	.	.	.	1	.	1
" Nail - - - -	.	.	.	.	.	1	.	1	.	1
Finger - - - -	.	.	1	.	.	.	.	.	1	1
Fire, Gas - - - -	.	.	.	1	.	.	.	1	.	1
FIREPLACE - - - -	VIIb	2	2	4	3	2	4	8	9	17
FISH, Ord. or unspd.	III	19	2	23	1	23	3	65	6	71
" Goldfish in bowl - -	.	.	1	.	1	.	5	.	7	7
Fishbone - - - -	.	.	.	.	.	1	.	1	.	1
FISHING ROD - - - -	.	.	3	1	1	1	.	2	4	6
Fish Slice - - - -	.	.	.	.	.	1	.	1	.	1
FLAG, Ord. or unspd.	II	4	4	7	3	1	4	12	11	23
" Black and/or crossed	IV	5	1	2	1	1	2	8	4	12
Flagstaff - - - -	.	1	1	.	.	.	.	1	1	2
Flask, Cheml. or - - -	.	1	.	2	.	.	1	3	1	4
" Chianti - - - -	.	.	1	.	.	.	.	.	1	1
Fleur de Lys - - - -	III	2	.	3	.	1	.	6	.	6
Flower, Indt. or unspd.	.	15	15	7	12	4	14	26	41	67
" Anemone - - - -	.	1	3	1	2	1	.	3	5	8
" Aster - - - -	.	1	.	1	.	.	.	2	.	2
" Bloodroot - - - -	.	1	.	.	.	.	.	1	.	1
" Bluebell - - - -	.	2	.	.	.	.	.	2	.	2
" Buttercup - - - -	.	1	.	2	1	2	.	5	1	6
" Canterbury Bell - - -	.	.	.	1	.	.	.	1	.	1
" Carnation - - - -	.	1	.	.	.	.	.	1	.	1
" Catmint - - - -	.	.	.	.	.	.	1	.	1	1
" Chicory - - - -	.	1	.	.	.	.	.	1	.	1
" Chrysanthemum - - -	.	.	1	1	4	1	1	2	6	8
" Cornflower - - - -	.	1	.	.	.	.	.	1	.	1
" Crocus - - - -	.	1	.	2	2	.	.	3	2	5
" Daffodil - - - -	VII*	2	3	5	3	8	6	15	12	27
" Dahlia - - - -	.	.	.	1	.	2	.	2	1	3
" Daisy - - - -	.	2	1	3	.	.	1	5	2	7
" " (doubtful) - - - -	.	2	1	4	.	1	1	7	2	9
" Dandelion - - - -	.	2	.	.	.	.	.	2	.	2
" Forgetmenot - - - -	.	.	.	.	.	.	2	.	2	2
" Fritillary - - - -	.	.	.	1	.	.	.	1	.	1
" Fruit Blossom, var.	.	1	.	.	2	.	1	1	3	4
" Geranium - - - -	.	.	1	.	1	.	.	.	2	2
" Harebell - - - -	.	.	.	.	.	.	1	.	1	1
" Heather, White - - -	.	.	1	.	.	.	.	.	1	1
" Hyacinth - - - -	.	.	1	.	2	.	.	.	3	3
" Hydrangea - - - -	.	.	.	.	.	.	1	.	1	1
" Iris - - - -	.	.	.	.	.	.	1	.	1	1
" Jonquil - - - -	.	.	.	1	.	.	.	1	.	1
" Lily, Arum - - - -	VIIc	.	1	.	.	1	.	1	1	2
" " Madonna - - - -	.	1	.	.	.	.	.	1	.	1
" " Tiger - - - -	.	.	.	.	.	1	.	1	.	1
" " of Valley - - - -	.	1	.	1	.	.	.	3	.	3
" " Water - - - -	.	.	1	.	.	3	1	3	2	5
" " Unspd. - - - -	.	1	.	.	.	.	.	1	.	1

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Name of OBJECT	Orgl. in Expt.	I-V		VI		VII		TOTALS		
		$\alpha$	$\beta$	$\alpha$	$\beta$	$\alpha$	$\beta$	$\alpha$	$\beta$	Total
Flower, Lupin - - -	.	1	.	.	.	2	1	2	1	3
" Marguerite - - -	.	1	1	2	.	1	3	4	4	8
" Marigold, Ord. - -	.	1	.	2	.	3	1	6	1	7
" " French - - -	.	2	.	.	.	.	.	2	.	2
" Nasturtium - - -	.	.	1	.	.	1	.	1	1	2
" Pansy - - -	VIIe	4	.	2	.	.	.	6	.	6
" Peony - - -	.	.	.	2	1	.	.	2	1	3
" Poppy - - -	.	1	.	1	3	2	.	4	3	7
" Primrose - - -	.	1	.	.	1	1	1	2	2	4
" Ragged Robin - -	.	1	.	.	.	.	.	1	.	1
" Rose - - -	.	8	2	5	.	2	3	15	5	20
" Stitchwort - - -	.	1	.	.	.	.	.	1	.	1
" Sunflower - - -	.	.	2	3	.	1	.	3	3	6
" Thistle - - -	VII*	4	.	.	.	1	.	4	1	5
" Tulip - - -	.	13	7	4	2	5	9	22	18	40
" Vetch - - -	.	.	.	.	.	1	.	.	1	1
" Violet - - -	.	1	.	1	.	.	.	2	.	2
FLY, House or unspd.	.	.	.	1	.	1	.	2	.	2
FOOT, Ord. - - -	VII*	4	1	2	.	.	.	6	1	7
" Winged - - -	.	.	.	1	.	.	.	1	.	1
Football, Rugby, game of	.	1	.	.	.	.	.	1	.	1
Footstool - - -	.	.	.	2	.	.	.	2	.	2
FORK, Garden - - -	.	.	.	1	.	4	.	5	.	5
" Table - - -	.	1	2	.	5	1	5	2	12	14
" Pitch - - -	.	.	.	1	.	.	.	1	.	1
FOUNTAIN - - -	VII*	.	.	3	2	2	.	5	2	7
FOX - - -	.	1	.	3	.	.	.	4	.	4
FROG - - -	III	2	.	2	.	5	.	9	.	9
Fruit, Indt. or unspd.	.	.	2	1	2	1	4	2	8	10
FRYING PAN - - -	VI*	2	.	1	.	1	.	4	.	4
FUNNEL (Cheml.) - -	VIIId	4	.	6	.	.	1	10	1	11
Galleon, See SHIP	.	.	.	.	.	.	.	.	.	.
Galley (Rowing) - -	.	1	.	2	.	.	.	3	.	3
GALLOWS or GIBBET -	VI*	2	1	.	1	.	.	2	2	4
Garden, Sp. Mn. - -	.	4	.	.	4	.	3	4	7	11
Gas-fire, See Fire	.	.	.	.	.	.	.	.	.	.
GAS-RING - - -	.	.	1	.	.	.	.	.	1	1
GATE, Ord. as in field	VI*	4	8	9	11	16	8	29	27	56
Gear, Mechl., unspd. -	.	.	1	.	.	.	.	.	1	1
Geyser (Natural) - -	.	.	.	.	.	1	.	1	.	1
Ghost, See Spook	.	.	.	.	.	.	.	.	.	.
GIBBET, See GALLOWS	.	.	.	.	.	.	.	.	.	.
Gimlet - - -	.	.	.	1	.	.	.	1	.	1
GIRAFFE - - -	VII*	4	.	3	1	1	.	8	1	9
Girl, See HUMAN BEINGS	.	.	.	.	.	.	.	.	.	.
GLOBE, Terrestrial - -	.	3	1	2	.	1	.	6	1	7
GLOVE - - -	VII*	2	1	1	.	2	2	5	3	8
Gnome - - -	.	1	.	.	.	1	.	2	.	2
Goal, Football or Hockey	.	.	3	.	1	.	2	.	6	6
GOAT - - -	VII*	1	.	1	1	4	.	6	1	7
Goblet - - -	II	4	.	4	.	1	.	9	.	9
" -shaped dish - - -	.	.	4	.	1	.	1	.	6	6
God, Garment of - -	.	.	.	1	.	.	.	1	.	1
Goggles - - -	.	.	1	.	.	.	.	.	1	1
GOLF CLUB - - -	VIIa	.	.	3	1	.	2	3	3	6
" Clubs, Bag of - -	.	1	.	.	1	.	1	1	2	3
Gondola, ~ Venetian	.	.	1	.	.	.	.	.	1	1



## Experiments on the Paranormal Cognition of Drawings

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Name of OBJECT	Orgl. in Expt.	I-V		VI		VII		TOTALS		
		$\alpha$	$\beta$	$\alpha$	$\beta$	$\alpha$	$\beta$	$\alpha$	$\beta$	Total
Gong - - - - -	.	.	.	1	.	.	.	1	.	1
Governor, Mechl. - - - -	.	.	.	1	.	.	.	1	.	1
Gown, Academic - - - -	.	.	2	.	.	.	.	.	2	2
GRAMOPHONE, Box type - -	.	2	.	.	.	.	.	2	.	2
" with Horn - - - -	.	.	.	.	.	1	.	1	.	1
" Radio - - - - -	.	.	.	.	.	2	.	2	.	2
GRAPES, Bunch of - - - -	VI*	4	4	3	5	3	7	10	16	26
Grape Fruit - - - - -	.	.	.	.	.	1	.	.	1	1
Grass, Sp. Mn. - - - - -	.	.	2	1	4	2	2	3	8	11
GRASSHOPPER - - - - -	VI*	.	.	1	.	.	.	1	.	1
Grave or Graveyard - - -	.	.	.	2	3	.	.	2	3	5
Greenhouse - - - - -	.	.	.	.	.	1	.	1	.	1
Grille - - - - -	.	1	.	.	1	.	.	1	1	2
GRINDSTONE or pr. - - -	VII*	1	.	.	.	.	.	1	.	1
Guillotine - - - - -	.	.	.	.	.	1	.	1	.	1
GUN, A.A. - - - - -	.	1	1	.	.	.	.	1	1	2
" Field, Heavy or Cannon	VII*	1	3	9	1	6	1	16	5	21
" Rifle - - - - -	.	.	1	3	.	2	.	5	1	6
" Sporting or ~ - - - -	II	1	1	1	1	.	1	2	3	5
" Top, unspd. - - - - -	.	1	.	.	.	.	.	1	.	1
Gyroscope - - - - -	.	1	.	2	.	.	.	3	.	3
Haddock, pr. dried - - -	.	.	.	.	.	1	.	1	.	1
HAIRPIN - - - - -	.	.	.	1	.	.	.	1	.	1
Halberd or ~ - - - - -	.	.	.	1	.	.	.	1	.	1
Halma, piece, or pr. - -	.	2	.	1	.	.	.	3	.	3
Halo - - - - -	.	1	.	1	.	.	.	1	1	2
Ham, pr. fried - - - - -	.	.	1	.	.	.	.	.	1	1
HAMMER - - - - -	V	2	1	4	1	2	.	8	2	10
HAMMOCK - - - - -	.	.	1	.	.	.	.	.	1	1
HAND - - - - -	I	16	6	10	2	4	5	30	13	43
Handkerchief - - - - -	.	.	1	.	.	1	.	.	2	2
Harness - - - - -	.	.	1	.	.	.	.	.	1	1
HARP - - - - -	VIIe	1	.	3	.	1	.	5	.	5
HAT, Fem., var. - - - -	.	2	.	1	5	3	3	6	8	14
" Masc., Bowler - - - -	.	.	1	3	1	1	.	3	3	6
" " Felt and var. - - -	VI*	4	4	6	2	4	.	14	6	20
" " Pierrot or - - - -	.	1	.	1	.	1	.	1	2	3
" " Pointed - - - - -	.	1	.	1	.	.	.	1	1	1
" " Straw ('Boater') - -	.	1	.	8	.	3	1	12	1	13
" " Top - - - - -	.	1	.	.	.	.	.	.	.	.
HATCHET, See CHOPPER - -	.	.	.	.	.	.	.	.	.	.
HAYSTACK - - - - -	.	3	.	4	.	2	.	3	6	9
Headdress, Red Indian - -	.	.	.	1	.	.	.	1	.	1
HEART, or convl. shape - -	.	4	.	1	2	1	.	6	2	8
Hedge - - - - -	.	.	4	2	.	4	.	10	.	10
HEDGEHOG - - - - -	.	1	.	1	.	1	.	3	.	3
Helix - - - - -	.	2	1	2	.	.	.	4	1	5
HELMET, ~ British steel -	.	.	1	3	1	1	.	4	2	6
" ~ German " - - - -	.	1	.	.	.	.	.	1	.	1
" Diver's - - - - -	.	.	1	1	.	.	.	1	1	2
" Fireman's - - - - -	.	.	.	1	.	.	.	1	.	1
" Knight's, var. - - - -	VII*	1	1	2	.	1	.	3	2	5
" Roman - - - - -	.	.	.	1	.	1	.	2	.	2
" Viking - - - - -	.	1	.	.	.	1	.	1	1	2
Hill, See Mountain - - -	.	.	.	.	.	1	.	2	.	2
HIPPOPOTAMUS - - - - -	.	1	.	.	.	.	.	.	1	1
Hips & Haws - - - - -	.	.	.	1	.	.	.	.	.	.

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Name of OBJECT	Orgl. in Expt.	I-V		VI		VII		TOTALS		
		$\alpha$	$\beta$	$\alpha$	$\beta$	$\alpha$	$\beta$	$\alpha$	$\beta$	Total
<i>Hockey Stick</i> - - -	.	.	.	.	.	1	2	1	2	3
<i>HOE (Garden)</i> - - -	.	1	1	.	.	.	.	1	1	2
<i>HOOK, Fish</i> - - -	.	.	1	.	.	1	.	.	2	2
" <i>Meat</i> - - -	.	.	.	.	.	1	.	.	1	1
" <i>Plain</i> - - -	.	.	.	1	.	.	.	.	1	1
<i>Hoop, Child's</i> - - -	.	.	3	1	.	1	.	.	5	5
<i>HORN, Drinking, on stand</i> -	.	.	.	1	.	.	.	1	.	1
" <i>Motor, See MOTOR</i>										
<b>HORN</b>										
<i>Horns, Animal, unspd.</i> - -		.	.	.	.	1	.	.	1	1
<i>HORSE, Ord.</i> - - -	II	18	6	12	14	16	12	46	32	78
" <i>Model or Toy</i> - - -	.	1	1	.	.	2	.	3	1	4
" <i>Rocking</i> - - -	.	1	.	.	.	1	.	1	1	2
" <i>Mask of head</i> - - -	.	.	.	1	.	.	.	1	.	1
<i>HORSESHOE</i> - - -	.	6	1	2	.	3	.	11	1	12
<i>Hose, Fire or ~</i> - - -	.	.	.	.	.	1	2	.	2	3
<i>HOOR-GLASS or EGG-TIMER</i> -	.	3	1	10	1	1	.	14	2	16
<i>HOUSE or COTTAGE, Ord.</i> -	VI	22	23	40	20	29	19	91	62	153
" <i>pl. Unspd.</i> - - -	.	1	3	.	3	.	1	1	7	8
" <i>Public or Inn</i> - - -	.	.	.	.	.	1	.	.	1	1
<i>Howdah</i> - - -	.	.	.	1	.	.	.	.	1	1
<i>Hub only of Wheel</i> - - -	.	1	.	.	.	.	.	1	.	1
<b>HUMAN BEINGS:</b>										
<i>Man, Youth, Boy, var.</i> -	.	14	49	5	49	6	48	25	146	171
" " <i>Head only</i> - - -	.	33	12	25	10	12	5	70	27	97
" " " <i>(Comic)</i> - - -	.	3	1	.	.	.	.	3	1	4
" <i>Archer</i> - - -	.	.	2	.	.	.	.	.	2	2
" <i>Aviator</i> - - -	.	.	1	.	.	.	.	.	1	1
" <i>Bishop</i> - - -	.	.	.	1	.	.	.	.	1	1
" <i>Cannibal</i> - - -	.	1	.	.	.	.	.	1	.	1
" <i>Chinaman</i> - - -	.	.	1	1	.	.	.	.	2	2
" <i>Clergyman</i> - - -	.	.	.	1	.	.	.	1	.	1
" <i>Clown</i> - - -	.	.	.	4	.	.	.	4	.	4
" <i>Cricketer</i> - - -	.	.	1	.	1	.	.	.	2	2
" <i>Cyclist</i> - - -	.	.	.	.	3	.	5	.	8	8
" <i>Diver, Deep sea</i> - - -	.	.	1	.	.	.	.	.	1	1
" <i>Dragoon</i> - - -	.	.	.	.	.	1	.	1	.	1
" <i>Fireman</i> - - -	.	.	.	.	.	.	2	.	2	2
" <i>Fisherman (Seafar- ing)</i> -	.	1	.	.	1	.	.	1	1	2
" <i>Fisherman (Angler)</i> -	.	.	4	.	2	.	1	.	7	7
" <i>Footballer</i> - - -	.	.	1	.	1	.	.	.	2	2
" <i>General</i> - - -	.	.	.	.	1	.	.	.	1	1
" <i>Herald</i> - - -	.	.	.	.	1	.	.	.	1	1
" <i>Indian (East)</i> - - -	.	.	.	.	.	1	.	.	1	1
" " <i>(Red)</i> - - -	.	1	1	.	4	1	.	2	5	7
" <i>Jester</i> - - -	.	1	.	.	.	.	.	1	.	1
" <i>Jockey</i> - - -	.	.	1	.	.	.	.	.	1	1
" <i>Knight</i> - - -	.	.	1	.	1	.	.	.	2	2
" <i>Monk</i> - - -	.	1	.	.	.	.	.	1	.	1
" <i>Native, unspd.</i> - - -	.	.	.	.	2	.	1	.	3	3
" <i>Negro</i> - - -	.	1	.	.	.	.	.	1	.	1
" <i>Organ-grinder</i> - - -	.	.	.	.	1	.	.	.	1	1
" <i>Parachutist</i> - - -	.	.	.	.	.	1	.	.	1	1
" <i>Piper (Scottish)</i> - - -	.	2	.	.	.	.	.	2	.	2
" <i>Policeman</i> - - -	.	.	1	.	.	.	.	.	1	1
" <i>Sailor (R.N.)</i> - - -	.	.	.	.	.	1	.	1	.	1
" <i>Schoolmaster or Don</i> -	.	.	1	1	2	.	.	1	3	4

Experiments on the Paranormal Cognition of Drawings

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Name of OBJECT	Orgl. in Expt.	I-V		VI		VII		TOTALS		
		$\alpha$	$\beta$	$\alpha$	$\beta$	$\alpha$	$\beta$	$\alpha$	$\beta$	Total
Man, Sentry - - -	.	.	.	.	1	.	.	.	1	1
" Shepherd - - -	.	.	2	.	3	.	1	.	6	6
" Skater (Ice or Roller) - - -	.	.	.	1	1	.	.	1	1	2
" Snake Charmer - - -	.	.	.	.	1	.	.	.	1	1
" Soldier, Indt. - - -	.	.	3	.	.	.	.	.	3	3
" Student or Scholar - - -	.	1	.	.	1	.	1	1	2	3
" Troops, unspd. - - -	.	.	.	.	2	.	1	.	3	3
" Tweedledum & -dee - - -	.	.	.	1	.	.	.	1	.	1
" Undergraduate - - -	.	1	1	.	.	.	.	1	1	2
" Viking - - -	.	.	.	.	.	.	1	.	1	1
" Warden, Air raid - - -	.	.	.	.	.	.	1	.	1	1
Woman or Girl, var. - - -	.	1	10	4	12	.	17	5	39	44
" " Head only - - -	.	9	1	6	3	4	3	19	7	26
" Britannia or ~ - - -	.	.	.	.	1	.	1	.	2	2
" Chinese - - -	.	.	.	1	.	.	.	1	.	1
" Dancer (Ballet, etc.) - - -	.	3	.	5	.	2	1	10	1	11
" Dutch - - -	.	.	.	1	2	1	1	2	3	5
" Egyptian - - -	.	1	.	.	1	.	.	1	1	2
" Madonna - - -	.	.	1	.	.	.	.	.	1	1
" Nude - - -	.	.	1	.	.	1	1	2	2	4
" Nun - - -	.	.	.	1	.	1	.	2	.	2
" Nurse, Child's - - -	.	.	.	.	1	.	.	.	1	1
" " Hospital - - -	.	1	.	1	.	.	.	2	.	2
" Red Riding Hood - - -	.	.	.	.	1	.	.	.	1	1
" Refugee - - -	.	.	.	1	.	.	.	1	.	1
" Witch - - - VI*	.	1	.	.	1	.	1	1	2	3
Couple (Man & Woman) - - -	.	2	3	.	1	2	2	4	6	10
Children, Baby - - -	.	1	2	2	2	.	2	3	6	9
" Child, M or unspd. - - -	.	.	3	1	3	.	3	1	9	10
" Child, Fem. - - -	.	3	3	1	9	1	7	5	19	24
" Two. M & F - - -	.	.	1	.	1	.	.	.	2	2
" Pl., var. - - -	.	3	.	.	1	.	1	3	2	5
Crowd, 'People,' etc. - - -	.	.	2	1	1	.	1	1	4	5
Humpty Dumpty - - -	.	.	1	.	.	.	.	.	1	1
Hut or Shed, var. - - -	.	2	3	4	5	1	2	7	10	17
Ice-Cream Cart (Tricycle) - - -	.	.	.	.	.	.	1	.	1	1
" " Cone - - -	.	.	.	1	.	1	.	2	.	2
IGLOO - - -	.	.	.	2	.	.	.	2	.	2
Infinity Sign - - -	.	.	.	1	.	.	.	1	.	1
Inkblot - - -	.	1	.	.	.	.	.	1	.	1
INK-POT or WELL - - - VII*	.	1	1	6	7	3	10	10	18	28
Insect, Indt. - - -	.	1	.	.	.	.	.	1	.	1
Insulator, Electric - - - VII*	.	1	.	.	.	.	.	1	.	1
IRON, Domestic - - -	.	2	.	1	.	1	.	4	.	4
Island, Sp. Mn. - - -	.	.	.	.	5	.	1	.	6	6
Jack-in-the-Box - - -	.	.	.	.	.	1	.	.	1	1
Jar ~ Jam - - -	.	.	1	6	.	3	1	9	2	11
" ~ Sphl., with lid - - -	.	1	.	.	.	.	.	1	.	1
JUG or EWER - - - V	.	17	1	17	2	13	5	47	8	55
KANGAROO - - -	.	.	.	.	.	1	.	1	.	1
KENNEL, Dog - - - VII*	.	1	1	2	1	.	3	3	5	8

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Name of OBJECT	Orgl. in Expt.	I-V		VI		VII		TOTALS		
		$\alpha$	$\beta$	$\alpha$	$\beta$	$\alpha$	$\beta$	$\alpha$	$\beta$	Total
KETTLE - - - - -	VI*	.	1	4	.	8	.	12	1	13
KEY, Ord. - - - - -	VIIc	10	1	9	.	10	.	29	1	30
" <i>Yale or special</i> - - - - -	.	.	.	2	.	1	.	3	.	3
Kitchen Range - - - - -	.	.	1	.	.	.	.	.	1	1
KITE - - - - -	VII*	4	1	3	1	4	2	11	4	15
KNIFE, Paper - - - - -	.	1	.	1	.	.	.	2	.	2
" Pen or Pocket - - - - -	.	1	.	4	.	1	.	6	.	6
" Table - - - - -	VIIb	.	4	5	8	8	9	13	21	34
Knot (in rope) - - - - -	.	1	.	.	.	.	.	1	.	1
LADDER - - - - -	VIIa	10	3	11	2	9	3	30	8	38
LADLE - - - - -	.	1	.	1	.	.	.	2	.	2
Lake or Loch - - - - -	.	.	5	.	14	.	.	.	26	26
LAMB - - - - -	.	1	.	.	1	2	3	3	4	7
LAMP, Cycle - - - - -	.	.	.	.	.	1	.	1	.	1
" <i>Electric</i> (See note) - - - - -	.	1	3	1	.	2	1	4	4	8
" <i>Floor, tall</i> - - - - -	.	3	.	1	.	.	.	4	.	4
" <i>Hanging, unspd.</i> - - - - -	.	1	1	.	.	.	.	1	1	2
" <i>Oil, Heating</i> - - - - -	.	.	.	2	.	.	.	2	.	2
" <i>Spirit, glass</i> - - - - -	.	.	.	.	.	1	.	1	.	1
" <i>Street</i> - - - - -	.	2	3	6	3	10	6	18	12	30
" <i>Table, var.</i> - - - - -	VI	5	2	18	4	4	3	27	9	36
Lampshade (No lamp) - - - - -	.	2	.	1	.	2	.	5	.	5
LANCE, See SPEAR										
LANTERN, Ord., Hand or hanging - - - - -	.	2	1	2	.	1	.	5	1	6
" <i>Projection</i> - - - - -	.	1	.	.	.	.	.	1	.	1
LATHE - - - - -	VII*	.	.	.	.	.	.	.	.	.
LAUNCH - - - - -	VII*	.	.	.	.	.	.	.	.	.
Lawn, Sp. Mn. - - - - -	.	.	1	.	.	.	1	.	2	2
Lead or Leash, Dog - - - - -	.	.	1	.	2	.	2	.	5	5
LEAF, single, var. - - - - -	II	4	1	11	1	7	1	22	3	25
" <i>~ 2 or 3 or few</i> - - - - -	.	7	2	5	1	1	.	13	3	16
" <i>Pl., unspd.</i> - - - - -	.	.	1	.	1	.	.	2	.	2
Lectern - - - - -	.	.	.	.	.	.	1	.	1	1
Leek - - - - -	.	1	.	.	.	.	.	1	.	1
Legs of Man, See Triskelion										
LETTER, Postal, See ENVELOPE										
Letter Rack - - - - -	.	.	.	1	.	.	.	1	.	1
LIFEBELT - - - - -	.	.	.	1	.	.	.	1	.	1
LIGHTHOUSE - - - - -	VI*	4	2	5	3	4	8	13	13	26
Lightning - - - - -	.	1	3	.	1	.	.	1	4	5
LINK, Chain or ~ - - - - -	.	1	.	.	.	1	.	2	.	2
" <i>Cuff</i> - - - - -	.	1	.	.	.	.	.	1	.	1
LION, Ord. - - - - -	.	2	.	1	.	4	.	7	.	7
" <i>Heraldic or ~</i> - - - - -	II	2	.	.	1	.	.	2	1	3
LIZARD - - - - -	.	1	1	.	.	.	.	1	1	2
LOAF - - - - -	VII*	4	1	3	3	10	7	17	11	28
LOBSTER or CRAYFISH - - - - -	.	2	.	.	.	2	.	4	.	4
LOCK, o.t., Padlock - - - - -	.	.	1	.	.	.	.	.	1	1
Locket - - - - -	.	.	.	.	1	.	.	.	1	1
LOCOMOTIVE, Ord., no train } " <i>with " }</i> " <i>Child's wooden</i> - - - - -	VIIe	5	.	7	1	10	.	22	1	23
Loop of String, etc. - - - - -	.	.	3	.	2	.	5	.	10	10
Luggage, Unspd. - - - - -	.	2	.	1	.	.	1	3	1	4
LYRE - - - - -	.	1	.	.	.	.	1	1	1	2
Machinery, unspd. - - - - -	.	1	.	.	.	.	.	1	.	1
MAGNIFYING GLASS - - - - -	.	1	.	.	.	1	.	2	.	2

Experiments on the Paranormal Cognition of Drawings

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Name of OBJECT	Orgl. in Expt.	I-V		VI		VII		TOTALS		
		α	β	α	β	α	β	α	β	Total
MALLET, Carpenter's - - -	.	1	.	2	.	.	.	3	.	3
Man, See HUMAN BEINGS										
Mandolin - - - - -	.	.	.	1	.	.	.	1	.	1
MANGLE, See WRINGER										
Map, var. - - - - -	.	6	.	4	.	3	.	13	.	13
Marker, Billiards - - -	.	.	.	1	.	.	.	.	1	1
MARROW, Veg., or pr. - -	.	1	.	2	.	1	.	3	1	4
MASK, Ord. or plain - -	.	2	.	1	.	1	.	4	.	4
" Death - - - - -	.	1	.	.	.	.	.	1	.	1
" Gas - - - - -	.	.	.	2	1	.	2	2	3	5
" Horse, See HORSE										
Mat, ornl., floor - - -	.	.	.	1	.	.	.	.	1	1
MATCH, not in box - - -	.	.	.	.	.	2	.	.	2	2
Meadow, See Field										
Medal - - - - -	.	1	.	.	.	.	.	1	.	1
Medusa - - - - -	.	.	.	1	.	.	.	1	.	1
MEGAPHONE - - - - -	VII*	.	.	2	.	1	.	2	1	3
Melon - - - - -	.	.	.	.	.	1	.	.	1	1
MERMAID - - - - -	.	.	.	.	.	.	.	1	.	1
METRONOME - - - - -	.	.	.	1	1	.	.	1	1	2
MICROSCOPE - - - - -	VII*	2	.	.	.	.	.	2	.	2
Milestone - - - - -	.	1	.	.	.	.	.	1	.	1
Milk 'Churn' or Can - -	.	.	.	.	.	1	1	1	1	2
Mill, water - - - - -	.	1	1	.	.	.	.	1	1	2
" " wheel of - - - -	.	1	1	.	.	.	.	1	1	2
MIRROR - - - - -	.	4	3	.	.	1	1	5	4	9
Mistletoe - - - - -	.	1	.	.	.	.	.	.	1	1
MITRE (Bishop's) - - -	.	1	.	1	1	1	.	3	1	4
Model (Artist's dummy) -	.	1	.	.	.	.	.	1	.	1
MONKEY or APE - - - -	VII*	3	1	3	1	2	.	8	2	10
MOON, Crescent - - - -	.	4	5	4	3	1	1	9	9	18
" Full or pr. - - - -	.	1	4	3	.	.	.	1	7	8
" Man in the - - - -	.	2	.	.	.	.	.	2	.	2
Moor or Moorland, Sp. mn.	.	.	2	.	.	.	.	.	2	2
Mop - - - - -	.	.	.	1	.	1	.	1	1	2
Mosque - - - - -	.	1	.	.	.	1	.	1	1	2
MOTORCAR,										
" Side view or unspd. }	VII*	4	.	7	4	8	1	19	5	24
" Front or rear view }	.	.	2	3	1	1	1	4	4	8
Motor Ambulance - - -	.	.	.	2	.	.	.	2	.	2
MOTOR BUS, See BUS.										
MOTOR CYCLE, solo - - -	.	.	.	.	.	1	.	.	1	1
" " with Sidecar - - -	.	.	.	.	.	1	.	1	.	1
MOTOR HORN (Bulb) - - -	VIIb	.	.	.	.	1	.	1	.	1
Mound, Sp. Mn. - - - -	.	2	.	.	.	.	.	.	2	2
Mountain or Hill, Ord. }	II	4	28	3	19	3	22	10	69	79
" " Notably peaked }	.	6	8	1	4	3	1	10	13	23
" " side, Sp. Mn. - - -	.	.	6	3	.	.	.	.	9	9
MOUSE or RAT - - - - -	VII*	3	.	4	1	5	3	12	4	16
MOUSETRAP - - - - -	.	.	.	.	.	2	.	.	2	2
Moustache, no face - - -	IV	.	.	.	.	.	.	.	.	.
Muff - - - - -	.	1	.	.	.	1	.	1	.	1
MUG or TANKARD - - - -	.	4	1	7	1	4	3	15	5	20
Mummy - - - - -	.	1	1	1	.	.	.	1	1	2
MUSHROOM or TOADSTOOL -	.	4	1	10	.	6	3	20	4	24
Musical Box - - - - -	.	.	.	1	.	.	.	1	.	1
" Clef, Stave, Note, etc.	.	4	1	4	2	.	.	8	3	11

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Name of OBJECT	Orgl. in Expt.	I-V		VI		VII		TOTALS		
		$\alpha$	$\beta$	$\alpha$	$\beta$	$\alpha$	$\beta$	$\alpha$	$\beta$	Total
<i>Music-holder</i> - - - -	.	1	.	.	1	.	.	1	1	2
<i>NAIL, Carpenter's</i> - - -	.	1	.	4	.	1	.	6	.	6
" <i>Finger</i> - - - -	.	1	.	.	.	.	.	1	.	1
<i>Napkin, Table</i> - - - -	.	.	1	.	.	.	.	.	1	1
<i>NECKLACE</i> - - - -	VII*	1	.	3	2	4	.	8	2	10
<i>NEEDLE, Ord., sewing</i> - -	.	.	.	.	1	.	1	.	2	2
" <i>Knitting</i> - - - -	.	.	.	.	1	.	2	.	3	3
<i>NEST, Bird's</i> - - - -	.	.	1	.	.	.	1	.	2	2
<i>NET, Fishing or ~</i> - - -	I	1	.	2	2	.	1	3	3	6
" <i>Landing or butterfly</i> -	.	.	.	.	1	.	2	2	1	3
" <i>Goal</i> - - - -	.	.	2	.	.	.	2	.	4	4
" <i>Tennis</i> - - - -	.	1	2	.	.	.	1	1	3	4
<i>Newspaper or Periodical</i> -	.	.	1	1	1	3	1	4	3	7
<i>Nightlight</i> - - - -	.	1	.	.	.	.	.	1	.	1
<i>Ninepin, See Skittle</i> - -	.	.	.	.	.	.	.	.	.	.
<i>NOSE</i> - - - -	.	1	.	1	.	.	.	2	.	2
<i>Noughts &amp; Crosses (Game)</i> -	.	1	1	.	.	.	.	1	1	2
<i>NUT, Ord.</i> - - - -	.	.	.	.	1	.	.	.	1	1
" <i>Coconut</i> - - - -	.	.	.	1	.	.	.	1	.	1
<i>NUTCRACKERS</i> - - - -	VII*	.	.	.	.	.	.	.	.	.
<i>OAR OF SCULL</i> - - - -	VII*	.	.	2	.	.	1	2	1	3
<i>Obelisk, ord., or Cenotaph</i> -	.	3	1	2	.	1	.	6	1	7
" <i>Cleopatra's Needle</i> - -	III	.	1	.	.	1	.	1	1	2
<i>OCTOPUS</i> - - - -	VII*	.	1	.	.	1	.	1	1	2
<i>OILCAN</i> - - - -	.	1	.	.	.	1	.	2	.	2
<i>ONION</i> - - - -	.	1	.	3	1	1	.	5	1	6
<i>ORANGE</i> - - - -	.	3	6	2	5	2	9	7	20	27
<i>Orb</i> - - - -	.	3	.	1	.	.	.	4	.	4
<i>Organ, Mouth</i> - - - -	.	.	.	.	.	1	.	1	.	1
" <i>Street</i> - - - -	.	.	.	.	.	.	1	.	1	1
<i>Ornaments, Brass, unspd.</i> -	.	.	1	.	.	.	.	.	1	1
<i>Overalls</i> - - - -	.	.	1	1	.	.	.	1	1	2
<i>Pad, Cricket</i> - - - -	.	.	.	.	1	.	.	.	1	1
<i>Paddle</i> - - - -	.	.	.	.	1	.	.	.	1	1
<i>PADLOCK</i> - - - -	VII*	.	.	.	1	.	.	.	1	1
<i>PAIL, See Bucket</i> - - - -	.	.	.	.	.	.	.	.	.	.
<i>Paint Box</i> - - - -	.	.	.	.	.	1	.	.	1	1
<i>PALETTE</i> - - - -	.	.	.	1	.	.	1	1	1	2
<i>Pan, Deep preserving</i> - -	.	1	.	.	.	.	.	1	.	1
<i>Panda</i> - - - -	.	.	.	.	.	1	.	1	.	1
<i>PANTHER</i> - - - -	.	.	.	.	.	1	.	.	1	1
<i>Pants, Under</i> - - - -	.	.	.	.	.	1	1	1	1	2
<i>Paper, Pile or sheet of</i> - -	.	2	.	.	.	.	4	2	4	6
<i>PAPER-CLIP, var.</i> - - - -	.	.	.	.	.	1	1	1	1	2
<i>Paperknife, See KNIFE</i> - -	.	.	.	.	.	.	.	.	.	.
<i>PARACHUTE</i> - - - -	.	3	.	1	.	1	3	5	3	8
<i>Parnassus</i> - - - -	II	.	.	.	.	.	.	.	.	.
<i>Pastry Board</i> - - - -	.	.	.	.	.	2	.	.	2	2
" <i>Cutter</i> - - - -	.	.	.	.	.	1	.	.	1	1
<i>Path</i> - - - -	.	1	8	1	5	.	4	2	17	19
<i>Paton</i> - - - -	.	.	.	1	.	.	.	1	.	1
<i>Peach</i> - - - -	.	1	.	.	.	.	.	1	.	1
<i>PEAR</i> - - - -	.	.	1	6	3	4	3	10	7	17
<i>Pebbles</i> - - - -	.	1	.	.	.	.	.	1	.	1
<i>PEN, Ord.</i> - - - -	.	1	1	4	3	5	5	10	9	19
" <i>Fountain</i> - - - -	VII*	.	.	4	1	3	.	7	1	8
" <i>Quill</i> - - - -	.	1	1	2	2	2	4	5	7	12

Name of OBJECT	Orgl. in Expt.	I-V		VI		VII		TOTALS		
		$\alpha$	$\beta$	$\alpha$	$\beta$	$\alpha$	$\beta$	$\alpha$	$\beta$	Total
Pen-nib (no pen)		1	.	.	.	.	.	1	.	1
Pen-tray		.	.	.	.	1	.	1	.	1
PENCIL, Ord.		4	1	7	2	8	2	19	5	24
" Propelling		.	.	1	.	.	.	1	.	1
Pendulum		.	.	1	.	1	.	1	1	2
Pepper Pot		.	.	1	.	.	.	.	1	1
PERAMBULATOR	VIIe	1	.	1	1	1	.	3	1	4
Pews (Church)		.	.	1	.	.	.	.	1	1
PIANO, Grand		1	.	3	.	.	.	4	.	4
" Upright or unspd.		3	.	2	2	1	.	6	2	8
PICKAXE	VIIId	2	.	3	.	1	.	6	.	6
PICTURE FRAME	VII*	4	2	2	4	3	5	9	11	20
Pie		.	.	1	.	1	.	.	2	2
Pier or Landing Stage		.	.	1	.	1	.	1	1	2
PIG, Incl. Boar, wild	VI*	2	.	3	1	5	2	10	3	13
PILLAR, See COLUMN		.	.	.	.	.	.	.	.	.
PILLAR-BOX (Postal)	VIIc	.	.	6	1	5	1	11	2	13
PIN, Ord.		.	.	1	.	.	.	1	.	1
" Hat		1	.	.	.	.	.	1	.	1
" Scarf or Tie		1	.	.	.	.	.	1	.	1
Pin Cushion		.	.	2	.	.	.	.	2	2
PINCERS	VIIb	1	.	2	.	.	.	3	.	3
PINEAPPLE		1	.	2	1	2	.	5	1	6
Pine Cone		.	.	1	.	.	.	.	1	1
PIPE ~ Drain, etc.		2	1	2	.	.	.	2	3	5
" Tobacco	VII*	11	1	13	3	10	3	34	7	41
Pipe-rack		.	.	1	.	.	.	1	.	1
PISTOL or REVOLVER, etc.	VII*	.	.	4	.	.	.	4	.	4
Plant, No flower, Indt.		1	3	6	.	3	.	1	12	13
" " Aspidistra		.	2	.	.	2	.	.	4	4
" " Cactus		1	.	2	1	1	.	2	3	5
" " Gorse		.	2	.	.	.	.	.	2	2
" " Ivy		.	1	.	.	.	.	.	1	1
Plate or Platter, Ord.		5	3	3	12	.	14	8	29	37
PLOUGH		1	1	.	1	.	1	1	3	4
Plum		.	1	.	.	.	.	.	1	1
Poem or Song		1	2	1	1	1	.	3	3	6
Pole, Punt or ~		.	1	.	1	.	1	.	3	3
Polecat or pr.		.	.	1	.	.	.	1	.	1
Pond or Pool		1	6	.	3	1	4	2	13	15
Porch		.	1	.	.	1	.	.	2	2
PORCUPINE	VII*	1	.	.	.	1	.	2	.	2
Portcullis		.	.	.	.	2	1	2	1	3
Post or Pole, fixed		.	1	.	.	.	2	.	3	3
Pot, Flower, empty		1	.	4	.	4	.	9	.	9
" " with plant, etc.		.	14	.	15	.	10	.	39	39
" Jam, See Jar		.	.	.	.	.	.	.	.	.
" Paint		.	1	.	.	.	.	.	1	1
Potato		1	.	1	.	1	1	3	1	4
Pouffe		.	.	1	1	1	.	2	1	3
PRAWN	II	.	.	.	.	.	.	.	.	.
PROPELLER, See AIRSCREW		.	.	.	.	.	.	.	.	.
Pudding		.	.	1	.	1	.	1	1	2
PUMP, Ord., ~ 'Village'	VII*	1	.	2	.	.	.	3	.	3
Punch-ball on Stand		1	.	.	.	.	.	1	.	1
Punt		.	.	1	.	1	.	.	2	2
Pylon, Electric		1	.	.	.	1	.	1	1	2





Name of OBJECT	Orgl. in Expt.	I-V		VI		VII		TOTALS		
		$\alpha$	$\beta$	$\alpha$	$\beta$	$\alpha$	$\beta$	$\alpha$	$\beta$	Total
Salt Sprinkler	-	1	.	1	.	.	.	1	1	2
Sand, Sp. Mn.	-	.	4	.	3	.	2	.	9	9
Sandal	-	.	1	.	.	.	.	.	1	1
SATURN	-	III	3	.	2	.	1	.	6	6
Sauceboat	-	.	1	.	.	.	.	1	.	1
SAUCEPAN	-	.	4	1	3	.	4	11	1	12
SAUCER, No Cup	-	.	1	.	.	.	5	.	6	6
" with Cup	-	.	10	.	16	.	24	.	50	50
SAUSAGE	-	.	.	.	1	.	3	.	4	4
SAW, Cardenter's	-	VII*	2	.	3	.	2	.	7	7
Saw-horse	-	.	1	.	.	.	1	.	2	2
SAXOPHONE	-	.	.	1	.	.	.	1	.	1
Scabbard	-	.	.	.	.	1	1	1	1	2
Scale, Graduated, o.t. Ruler	-	.	.	1	.	.	.	1	.	1
Scarecrow	-	.	.	1	.	.	.	1	.	1
SCARF	-	.	.	1	1	1	.	1	1	2
SCISSORS or SHEARS	-	IV	9	.	13	.	6	28	.	28
Scone	-	.	.	.	.	1	.	1	.	1
SCOOTER, Child's	-	VII*	.	1	.	1	.	1	1	2
Screen, unspd.	-	.	.	.	.	1	.	.	1	1
SCREWDRIVER	-	VIIa	.	.	.	.	.	.	.	.
Scroll, Iron, Sp. Mn.	-	.	.	.	1	.	.	.	1	1
" Paper, etc.	-	.	1	1	.	2	1	2	3	5
" Stonework	-	.	.	.	.	1	.	1	.	1
SCYTHE	-	.	1	.	.	.	.	1	1	2
Sea, Sp. Mn.	-	.	11	1	10	.	16	1	37	38
Sea Anemone	-	.	.	.	.	1	.	1	.	1
Sea Horse	-	.	1	.	.	.	1	1	1	2
SEAL (Animal)	-	.	.	.	.	1	1	1	1	2
SEE-SAW	-	VII*	.	1	.	1	.	.	3	3
SENTRY BOX	-	.	.	.	1	.	.	.	1	1
SEWING MACHINE	-	VI*	.	.	1	.	.	1	.	1
Shaving Cream, Tube of	-	.	1	.	.	.	.	.	1	1
Shovel	-	.	1	.	2	.	1	.	4	4
SHEAF of Wheat, Corn, etc.	-	.	2	.	1	1	2	5	1	6
Sheath, Cyl., for Hook	-	.	.	.	1	.	.	.	1	1
SHEEP or RAM, single or ~	-	VIIb	3	.	2	.	3	1	8	1
" " pl. or flock	-	.	5	.	3	.	1	.	9	9
SHELL (Artillery)	-	.	1	.	.	1	.	1	1	2
" (Conchological)	-	V	5	.	3	.	5	13	.	13
Shelter, Air Raid	-	.	.	1	1	.	.	1	1	2
SHIELD	-	VI*	1	4	2	3	1	2	4	9
SHIP, Indt. or unspd.	-	.	2	2	2	2	1	.	5	4
" Sailing, Ord.	-	.	3	.	11	1	9	23	1	24
" " ~ Galleon	-	.	1	1	1	1	2	2	4	6
" Steam, Ord.	-	.	4	6	16	3	7	6	27	15
" " War	-	.	1	.	2	1	1	.	4	1
SHIRT	-	.	1	.	.	.	.	.	1	1
SHOE, Laced, ord. M.	-	.	2	.	7	1	10	2	19	3
" F., ord., high-heeled or walking	-	.	4	.	1	.	4	2	9	2
" ~ bedroom slipper	-	.	.	1	4	.	1	.	5	1
" Child's	-	.	.	.	.	1	.	1	.	1
Shoe Tree	-	.	.	.	.	2	.	2	.	2
Shooting, Word (See note)	-	II	.	1	.	.	.	.	1	1
" Incident	-	.	.	1	.	.	.	.	1	1
SHOVEL, Ord., See SPADE	-	.	.	.	.	.	.	.	.	.
" for Incendiary bomb	-	.	.	.	.	.	1	.	1	1
Shredded Wheat	-	.	.	.	.	.	1	.	1	1

Name of OBJECT	Orgl. in Expt.	I-V		VI		VII		TOTALS		
		$\alpha$	$\beta$	$\alpha$	$\beta$	$\alpha$	$\beta$	$\alpha$	$\beta$	Total
<i>Shrub, See Bush</i>										
SHUTTLECOCK - - - -	VII*		1						1	1
SICKLE - - - -	VII*	2	1	1	1		1	3	3	6
<i>Side-car, See MOTOR CYCLE</i>										
Sign, Inn - - - -							1		1	1
SIGNAL, Railway - - -				2				2		2
SIGNPOST - - - -		6		1		2	1	9	1	10
SIPHON, Soda - - - -	VII*	1		1				1	1	2
Siren, Air Raid - - -							1		1	1
SKATE, Ice - - - -	VII*			1				1		1
" Roller - - - -					1				1	1
SKELETON - - - -			2		1	1		1	3	4
Ski - - - -				1			2	1	2	3
" Sticks - - - -							2		2	2
SKIPPING ROPE - - -					1			1	1	2
Skittle or Ninepin - -		1				2		3		3
SKULL - - - -	III	1	2	2			2	3	4	7
Slabs, Concrete, inscribed			1						1	1
SLEDGE OF SLEIGH - -		1						1		1
Slime, Sp. Mn. - - -		1						1		1
Smock - - - -			2						2	2
Smoke, Sp. Mn. - - -			1						1	1
SNAIL - - - -		6		4				14		14
SNAKE - - - -		6	2	3	1	6	1	15	4	19
Snow, Sp. Mn. - - -			2						2	2
Snow-man - - - -		1		1		1		3		3
SOAP, Cake of - - -			1	1		1	1	2	2	4
SOCK or STOCKING - -		1	1	1			2	2	3	5
SOFA or COUCH - - -	VI*	1	3				2	1	5	6
Song, See Poem										
SPODE, Ord. or SHOVEL -	VII*	6	1	6	1	8	4	20	6	26
" Conv'l., as in cards		2						2		2
Spanner, Adjustable - -	VIIe	1		1				2		2
" Fixed - - - -		1						1		1
SPEAR OF LANCE - - -			1		3		3		7	7
SPECTACLES, Ord. - - -	III	5	2	14	3	8	2	27	7	34
" Pincenez - - - -		2		1	1	1		4	1	5
SPHINX - - - -					1	1	1	1	2	3
SPIDER - - - -		2	3			3		5	3	8
" Web - - - -		1	3	1				2	3	5
SPINNING TOP - - -	II	2		1		4	1	7	1	8
SPINNING WHEEL - - -			1		1	1	1	1	3	4
Spiral, ~ Geoml. - - -		6	1	2		1		9	1	10
Spire or Steeple - - -		1	2	1	2	2	1	4	5	9
Splash - - - -		1				1		2		2
Sponge-rack - - - -							1		1	1
Spook or pr. - - - -			1						1	1
SPOON - - - -	VII*	4	1	6	4	8	5	18	10	28
Spring, Helical - - -	VIIa	2		3	1	1		6	1	7
" Spiral - - - -		1						1		1
SPUR - - - -	VII*	1	1			1		2	1	3
Square, Carprs. or Mason's		1	1	1				2	1	3
" Draughtsman's - -		2						2		2
SQUIRREL - - - -		1		2				3		3
STAG or DEER - - - -	VII*	2	1	1		1	1	4	2	6
" Head only, mounted			1						1	1
Staircase, Moving - -							1		1	1
Stairs, Ord. - - - -			3	3	1		1	3	5	8

[illegible]

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Name of OBJECT	Orgl. in Expt.	I-V		VI		VII		TOTALS		
		$\alpha$	$\beta$	$\alpha$	$\beta$	$\alpha$	$\beta$	$\alpha$	$\beta$	Total
TAP, See STOP-COCK										
TAPE MEASURE - - - -	.	.	.	.	.	1	.	1	.	1
Target or Butt - - - -	.	1	.	1	3	2	.	1	6	7
TASSEL - - - - -	.	.	.	2	.	.	.	2	.	2
TEA COSY - - - - -	.	.	.	.	.	1	.	1	.	1
Tea Infuser - - - - -	.	.	.	.	.	1	.	1	.	1
TEAPOT - - - - -	VI	6	.	14	1	11	2	31	3	34
Tea Trolley - - - - -	.	1	.	.	.	.	.	1	.	1
TELEGRAPH POLE - - - -	.	2	2	2	3	8	1	12	6	18
TELEPHONE { Cradle }	VIIe	1	.	2	2	1	1	4	3	7
{ Upright }		3	.	4	.	3	.	10	.	10
TELESCOPE - - - - -	.	2	.	.	1	.	.	2	1	3
Temple, Greek or ~ - - -	II	4	1	2	1	1	.	7	2	9
" Mexican - - - - -	.	.	.	1	.	.	.	.	1	1
TENT - - - - -	VIIa	3	1	3	.	4	.	10	4	14
Terrace, Unspd. - - - -	.	.	1	.	.	.	.	.	1	1
THERMOMETER - - - - -	VIIe	.	.	.	.	.	.	.	.	.
THERMOS FLASK - - - - -	VIIc	1	.	1	.	.	.	2	.	2
THIMBLE - - - - -	.	.	.	.	.	2	1	2	1	3
Thorn - - - - -	.	.	1	.	.	.	.	.	1	1
Thread, o.t. on reel - - -	.	.	.	.	.	2	.	2	.	2
Threshing Machine - - -	.	1	.	.	.	.	.	1	.	1
Throne - - - - -	II	.	1	.	1	.	.	2	.	2
Thumb - - - - -	.	.	.	.	.	1	.	1	.	1
TIE, Sailor knot - - - -	.	.	.	3	.	2	.	5	.	5
TIGER - - - - -	VII*	.	.	.	1	2	1	2	2	4
Tiles, Black and White -	.	.	.	2	.	.	.	2	.	2
" Roof - - - - -	.	1	.	.	.	.	.	1	.	1
Timber, Pile of - - - -	.	1	.	.	.	.	.	1	.	1
TIN OPENER - - - - -	.	.	1	.	.	.	.	.	1	1
TOAD - - - - -	.	.	.	1	.	.	.	1	.	1
TOADSTOOL or FUNGUS, See										
MUSHROOM										
Toast, Piece of - - - - -	.	.	.	.	.	1	.	1	.	1
TOAST RACK - - - - -	VII*	.	.	.	.	.	.	.	.	.
TOMATO - - - - -	.	1	.	1	.	2	3	4	3	7
Tombstone or pr. - - - -	.	1	1	.	.	.	.	1	1	2
Tongs, Fire - - - - -	.	.	.	.	.	1	.	1	.	1
" Ice - - - - -	.	.	.	1	.	.	.	1	.	1
" Sugar - - - - -	.	1	.	.	.	2	.	3	.	3
TOOTH or TEETH - - - - -	.	1	.	.	.	1	.	2	.	2
TOOTHBRUSH, See BRUSH										
TOP, Spinning, See SPINNING										
TORCH, Ord. or unspd. - )	VII*	.	.	3	.	.	.	3	.	3
" Electric - - - - -		.	.	2	.	.	.	2	.	2
" School sign or ~ -		1	.	1	.	.	.	2	.	2
TORTOISE - - - - -	.	2	.	4	.	.	.	6	.	6
Totem Pole - - - - -	.	.	1	.	.	.	.	.	1	1
Towel - - - - -	.	.	.	.	.	1	1	1	1	2
Towel-horse - - - - -	.	.	.	.	.	1	.	1	.	1
Tower, var. - - - - -	.	7	1	7	2	3	.	17	3	20
Tractor, Electric - - - -	.	.	.	1	.	.	.	.	.	1
Traffic Sign or Light - -	.	2	1	1	.	1	.	4	1	5
Trailer, Motor car - - -	.	.	.	.	1	.	.	.	1	1
Train, with Locomotive -	.	.	4	.	2	.	5	.	11	11
" no " - - - - -	.	1	.	.	.	1	.	1	1	2
" Diesel - - - - -	.	.	.	1	.	.	.	.	.	1
Troin, ~ of Royal Robe -	.	.	.	1	.	.	.	1	.	1

## Experiments on the Paranormal Cognition of Drawings

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Name of OBJECT	Orgl. in Expt.	I-V		VI		VII		TOTALS		
		$\alpha$	$\beta$	$\alpha$	$\beta$	$\alpha$	$\beta$	$\alpha$	$\beta$	Total
TRAMCAR - - - -	.	.	.	1	.	1	.	2	.	2
Tray, Document - - -	.	.	.	1	.	.	.	.	1	1
" Household - - -	.	.	.	1	.	3	.	4	.	4
" Sand, as for Cat - -	.	.	.	.	.	1	.	1	.	1
TREE, Ord., convl. or unspd. -	.	31	25	26	20	26	26	83	71	154
" Chestnut - - - -	V	2	.	1	.	.	.	1	.	1
" Oak - - - -	.	2	.	1	1	2	.	5	1	6
" Sycamore - - - -	.	.	1	.	.	1	.	1	1	2
" Ord., etc., pl. - - -	.	3	15	.	9	1	9	4	33	37
" Conifer - - - -	.	4	7	10	7	5	2	19	16	35
" " pl. - - - -	.	1	6	.	.	1	1	2	7	9
" Bare or ~ - - - -	.	3	4	2	1	4	4	9	9	18
" Fruit - - - -	.	6	1	1	3	1	4	8	8	16
" Palm - - - -	.	2	3	5	9	5	5	12	17	29
" Pollard or ~, var. - -	.	2	.	.	.	2	.	4	.	4
" Poplar - - - -	.	.	3	1	2	1	2	2	7	9
" Willow - - - -	.	1	2	.	1	.	1	1	4	5
" Toy - - - -	.	2	1	.	.	.	.	2	1	3
" Xmas - - - -	.	1	2	3	3	1	2	5	7	12
" Avenue or ~ - - -	.	2	1	3	2	1	.	6	3	9
" Wood or Forest - - -	.	.	2	1	3	1	4	2	9	11
Trefoil - - - -	.	2	.	1	.	1	1	4	1	5
Trellis or Lattice - - -	.	.	1	1	1	.	.	1	2	3
Trench, Military - - -	.	.	1	.	.	.	.	.	1	1
Tricycle - - - -	.	1	.	1	.	.	.	2	.	2
Trident or ~ - - - -	V	4	1	3	1	.	.	7	2	9
Tripod, Cheml. - - - -	.	.	.	.	.	1	.	1	.	1
Triskelion - - - -	.	.	.	1	.	.	.	1	.	1
Trough, Horse - - - -	.	.	.	1	.	1	.	2	.	2
" Pig - - - -	.	.	.	.	.	1	.	1	.	1
TROUSERS - - - -	VI*	1	3	1	.	.	1	2	4	6
TROWEL, Brick - - - -	.	.	.	2	.	.	.	2	.	2
TRUMPET - - - -	.	.	.	2	1	3	.	5	1	6
Trunk, o.t. growing Tree -	.	.	1	.	.	.	2	.	3	3
TUB, Wash or ~ - - - -	.	.	.	1	.	.	.	2	.	2
TUBE, Unspd. - - - -	.	.	.	1	.	1	.	1	1	2
" Test - - - -	.	.	.	1	.	.	.	.	.	.
TUMBLER, Glass - - - -	VIIe	12	5	12	4	8	4	32	13	45
" Cheml. Beaker - - -	.	.	.	.	.	1	.	1	.	1
Tunnel (Road subway) - -	.	.	.	.	.	1	.	1	.	1
Turban - - - -	.	.	.	.	.	1	.	1	.	1
Turnip - - - -	.	1	.	.	.	1	.	2	.	2
Turnstile - - - -	.	.	.	.	.	1	.	1	.	1
Tyre - - - -	.	.	.	.	.	1	.	1	.	1
U-BOAT, See SUBMARINE										
UMBRELLA or PARASOL,										
~ Closed } VII*		7	.	5	1	12	4	24	5	29
~ Open } VII*		7	6	16	6	11	2	34	14	48
UNICORN - - - -	VIIId	2	.	1	.	.	.	3	.	3
VASE, var., empty - - -	.	20	.	20	5	16	.	56	5	61
" " with flowers, etc. -	.	.	14	.	23	.	21	.	58	58
" 2-handled, ~ Amphora -	.	8	1	2	1	1	.	11	2	13
Vault - - - -	.	.	1	.	.	.	.	.	1	1
Vegetables, Unspd. - - -	.	.	.	.	.	1	.	1	.	1
Viaduct - - - -	.	2	.	.	.	3	1	5	1	6
VIOLIN - - - -	III	3	.	5	.	3	2	11	2	13

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Name of OBJECT	Orgl. in Expt.	I-V		VI		VII		TOTALS		
		$\alpha$	$\beta$	$\alpha$	$\beta$	$\alpha$	$\beta$	$\alpha$	$\beta$	Total
<i>Viscera, Indt.</i> - - - -				1	.			1	.	1
VOLCANO - - - - -	VIIId	3	.	2	.	3	.	8	.	8
Wall - - - - -		1	9	.	4	2	12	3	25	28
Ward, ~ <i>Fairy's</i> - - -		.	.	.	.	.	1	.	1	1
WARDROBE OF ~ - - -		.	.	2	.	2	.	4	.	4
WASP - - - - -		.	.	.	.	1	.	1	.	1
WATCH - - - - -		2	1	2	.	4	1	8	2	10
Water, Outdoor, Unspd., Sp.										
<i>Mn.</i> - - - - -		1	11	1	3	.	2	2	16	18
<i>Jet of, from hose</i> -		.	.	.	.	.	1	.	1	1
WATER-BUTT, See BARREL										
WATERFALL - - - - -		4	6	.	2	2	1	6	9	15
WATERING CAN - - - -	VIIe	3	.	2	.	11	.	16	.	16
Wave or Ripple, Sp. Mn. -		2	4	.	2	1	1	3	7	10
Weathercock - - - - -		.	1	2	.	.	1	2	2	4
Weir - - - - -		.	1	.	.	.	.	.	1	1
Well-Head - - - - -		1	1	2	2	1	.	4	3	7
WHALE - - - - -	VII*	1	.	1	.	.	.	2	.	2
Wharf, See Quay										
WHEEL, Ord., ~ Cart - -	VII*	7	1	5	.	6	3	18	4	22
" Car Steering - - -		.	.	1	.	.	1	1	1	2
" Ship's Steering - -		2	.	1	.	.	.	3	.	3
" Toothed - - - - -		1	.	1	.	.	.	2	.	2
WHEELBARROW - - - - -	VII*	6	1	5	1	4	4	15	6	21
WHIP - - - - -		.	.	.	.	.	1	.	1	1
WHISTLE - - - - -		.	.	.	.	1	.	1	.	1
Wicket (Stumps) - ( -	VII*	.	1	1	1	.	1	1	3	4
Wig - - - - -		.	1	.	.	.	.	.	1	1
Wigwam - - - - -		1	.	2	2	.	1	3	3	6
Wind, Sp. Mn. - - - -		.	5	.	1	1	.	1	6	7
WINDMILL - - - - -	V	7	3	18	3	6	3	31	9	40
WINDOW, ~ Church, etc. -		2	2	3	2	.	.	5	4	9
" Domestic - - - -		3	7	1	8	5	19	9	34	43
" Shop - - - - -		.	.	.	.	1	1	1	1	2
Wineglass (Possibly Egg-cup)		2	.	1	.	1	.	4	.	4
(Not " ) - - - - -		5	2	3	3	5	.	13	5	18
Wings, Unspd. - - - -		3	.	.	.	1	.	4	.	4
Winning Post - - - - -		.	.	1	.	.	.	1	.	1
WIRELESS SET - - - - -		.	.	2	2	6	1	8	3	11
Wishbone - - - - -		.	.	1	.	.	.	1	.	1
WOLF - - - - -		.	.	.	2	.	.	.	2	2
WOMAN, See HUMAN BEING										
Wood or Forest, See TREE										
WOOL, See BALL										
WORM - - - - -		.	.	.	.	2	.	2	.	2
Wreath - - - - -		.	2	.	.	.	.	.	2	2
WRENCH, See SPANNER										
WRINGER OR MANGLE - - -	VIIId	.	.	.	.	.	.	.	.	.
ZEBRA - - - - -	VI*	.	.	1	.	1	.	2	.	2
ZIPP FASTENER OF ZIPPER -		.	1	.	1	.	.	.	2	2



## Catalogue I - VII: Summary

			I-V		VI		VII		TOTALS	
			$\alpha$	$\beta$	$\alpha$	$\beta$	$\alpha$	$\beta$	$\alpha$	$\beta$
A. ROMAN type items.										
Total	Headings	556								
"	Entries, $\alpha$ :		1,209		1,430		1,244		3,883	
"	" $\beta$ :			586		703		897		2,186
"	" $\alpha + \beta$ :		1,795		2,133		2,141		6,069	
B. <i>Italic</i> type items.										
Total	Headings	703								
"	Entries, $\alpha$ :		547		473		333		1,353	
"	" $\beta$ :			649		598		626		1,873
"	" $\alpha + \beta$ :		1,196		1,071		959		3,226	
C. ROMAN and <i>Italic</i> combined.										
Total	Headings	1,259								
"	Entries, $\alpha$ :		1,756		1,903		1,577		5,236	
"	" $\beta$ :			1,235		1,301		1,523		4,059
"	" $\alpha + \beta$ :		2,991		3,204		3,100		9,295	
D. Illegitimate or Doubtful.										
Total	Headings	114								
"	Entries, $\alpha$ :		108		69		42		219	
"	" $\beta$ :			243		186		184		613
"	" $\alpha + \beta$ :		351		255		226		832	
E. Net Relevant Items.										
Total	Headings	1,145								
"	Entries, $\alpha$ :		1,648		1,834		1,535		5,017	
"	" $\beta$ :			992		1,115		1,339		3,446
"	" $\alpha + \beta$ :		2,640		2,949		2,874		8,463	
Ratio $\alpha/\beta$			1.66		1.64		1.15		1.46	
Number of percipients			250		246		245		741	
Items per percipient			10.56		11.99		11.73		11.42	
F. Indeterminate, Geometrical, etc.										
Indeterminate			292		78		21		391	
Geometrical			207		33		30		270	
Letters of Alphabet			25		7		3		35	
Numbers or Numerals			11		4		0		15	
Total			535		122		54		711	
GROSS GRAND TOTAL			3,526		3,326		3,154		10,006	

## Catalogue of Frequencies Cross References and Notes

N.B. The name of the object in respect of which the Cross-reference or Note is made is given in small Roman type regardless of whatever it is so printed in the body of the Catalogue, or italicized. The names of similar or connected objects to which the reader is referred follow immediately in ordinary type with initial capitals. The notes, if any, apply to the object in Roman type only, unless otherwise stated.

For abbreviations used, see special list on page 66.

- ACE OF CLUBS: Card, Trefoil.  
ALIDADE: This is a replicate hit on Ruler.  
ANCESTOR: Listed only because an O for II. Ill'd by an Old Man leaning on Staff. The hit entered is "A man bent with age."  
ANGEL: Foot (winged), Sylph, Devil, God. Includes Cherubs.  
APPLE: Includes baskets of Apples, etc.  
ARM: Only if sp. mn. or very conspicuous, as in two cases of an Arm and Hand brandishing a Sword.  
ARMOUR: Knight, Helmet.  
ARROW: Dart. 'Full' means complete with point and feather even if only conventionally indicated. 'Partial' means more or less arrow-shaped marks, etc.  
AXE: Chopper; Halberd.  
BAG: Rucksack; Sack.  
BALANCE: Incl. one steelyard.  
BALLOONS, Child's, bunch of: Three, in VI, may have been partly determined by the Grapes of VI\*. Take c and N as 0 and 495 in case this is so.  
BALUSTRADE: Banisters. As of Balcony, etc. not Banisters.  
BAMBOOS: Reeds.  
BANISTERS: Balustrade. Not merely incidental to Stairs.  
BANJO: Violin, Mandolin, etc.  
BARGE: Boat.  
BARROW: Cart; Wheelbarrow.  
BAT (Baseball): Club.  
BATH, Foot: Pan.  
BATTLEMENTS: Not including Battlements on Castles, etc. Those listed are drawn without any supporting building.  
BAYONET: Dagger.  
BEACON, Belisha: Traffic Sign, Stop Sign.  
BELT: Strap.  
BENCH: Sofa. Ill'd by Garden type seat with back and arms, not school 'form.'  
BICYCLE: Tricycle.  
BLACKBOARD: All on Easels, separately listed.  
BOAT: Barge; Canoe; Galley; Gondola; Punt; Ship.  
BONE(s): Crossbones; Fishbone; Skeleton; Wishbone.  
BONFIRE: Fireplace.  
BOTTLE: Carafe; Decanter; Flask; Thermos. Milk bottles of VII may have been partly determined by Thermos of VIIc. Take c and N as 4 and 496 in case this is so.  
BOW: (Ribbon): Knot; Tie.  
BOWL,  $\frac{3}{4}$ -sphl, with Goldfish: Goldfish are separately listed.  
BOX, Cylindrical: Includes 'Tins.'  
BOX, Rectlr, closed: Case. Not mere geometrical cubes.  
BOX, Rectlr, with lid open: Gramophone.  
BRIDGE: Viaduct. Unspecified bridges are taken as Arched.

BRIM: Goblet. The O for II depicted a sort of Goblet with heavy rim. The D here listed is a perfect hit on the notion of 'brim,' showing only the upper edge of an uncompleted container.

BUDS: Leaf.

BUFFALO: The O for I was not unmistakably a Buffalo; Cows, etc., have always been accepted as hits.

BUGLE: Trumpet; Horn; other wind instruments.

BUILDINGS: Greenhouse; Hut.

BULBS, Electric: Does not include bulbs in Lamps, electric, hanging or wall.

BULBS, Geissler Potash: Only included because O for V.

BULBS, Glass: Sometimes rather obscure, but description seems applicable.

BUSH: Plant.

CABBAGE: One D in VI is described as "Cabbage or Lettuce" (n.s.l.)

CAKE: Loaf; Scone.

CANAL: River; Stream.

CANDLE: Nightlight.

CAPE: Coat; Cloak.

CARAFE: Bottle; Decanter; Flask.

CARAVAN: Cart.

CARPET: Mat; Rug.

CARRIAGE: Cart; Coach.

CARROT: Radish.

CART: Caravan; Carriage.

CASE: Box; Bag.

CASTLE (Chess): Chessmen.

CATAPULT: Wishbone. No actual catapult is drawn. The two entries are of indeterminate objects of Y shape very similar to that of catapult.

CATERPILLAR: Worm.

CAULDRON: Coal scuttle.

CHAIN: Link.

CHAIR, Markedly ceremonial: These are approximately Thrones, q.v.

CHESSBOARD: Castle; Pawn; Tiles. Includes reference to Chess.

CHIMNEY, Domestic: Not chimneys merely incidental to houses.

CHOPPER: Axe.

CHURCH: Spire.

CLARINET: Trumpet and other wind instruments.

CLOCK, Face only: Includes Clocks in church towers.

CLUB: Bat.

COACH: Carriage.

COAL SCUTTLE: Cauldron.

COAT or JACKET: Gown; Cape; Smock. Includes Blazer, Tunic.

COFFIN: Corpses; Grave; Mummy; Tombstone.

COIN: Medal.

COLUMN: Not columns forming part of building separately listed, e.g., Temple; these are single or virtually so.

COMET: Not definitely Comets, but of very strongly similar shape.

CONE: Ice cream.

CORK: Corks in bottles are not listed unless sp. mn.

CORKSCREW: Gimlet. O of VII was drawn as Gimlet though marked Cork-screw.

CORPSES: Coffin; Graves; Mummy.

COTTON APHID: Included only because O for III; no plausible hit.

COUNTERPANE: Rug.

COW: Includes Bull, Calf, Heifer, etc.

CRINOLINE: Dress. These are either sp. mn. or sufficiently conspicuous to justify departure from the rule of taking clothes for granted.

CROCODILE: Lizard.

CROMLECH: Obelisk.

CROOK: Stick.

CROSS, Latin or Greek: These are drawn in outline only, without indications of solidity, and excluding 'plus-sign' crosses, q.v.

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CROSS, St. Andrews: Does not include 'multiplication signs'.

CROSS, 'plus-sign': does not include small crosses incidental to churches.

CROSSBONE: Bone.

CROSS-STITCH: Only included because O for I. Only plausible candidates are mentions of Embroidery and of Stitch, q.v.

CROWN: Wreath. Includes Diadem and Coronet; also two of Laurel, specifically described as 'Crown'.

CRUET: Pepperpot; Salt Sprinkler.

CRYSTAL: Stone, precious.

CUP-and-SAUCER: In view of the fact that the O for VIIId was a Cup without a saucer, it seems preferable to treat cups standing on saucers as constituting single entities.

CURTAIN: Includes one or two instances of Drapery not on figures.

CUSHION: Pincushion; Pouffe.

DAGGER: Bayonet; Sword.

DARTBOARD: Target.

DECANTER: Bottle; Carafe; Flask.

DEVIL: Angel.

DISH, Shallow: Plate.

" ~ 1/5 deep: Bowl. ~ 1/5 deep means of depth about 1/5 of the diameter: similarly for following entry.

DITCH: Trench.

DIVIDERS: Scissors.

DOCK: Pier; Quay.

DOG: Includes a few cases of Hounds and one stuffed dog.

DRAWING BOARD: Pastry board.

DRESS: Crinoline. Includes Skirt.

EAR: Not parts of faces even if sp. mn.

EGG: Not mere ovals or ellipses, which would be excluded as geoml.

EGG-CUP: Wineglass.

EMBROIDERY: Cross-stitch.

ENVELOPE: Not mere square or rectangle with diagonals.

EXFOLIATE: A bad O for II, illustrated by a drawing of one large leaf with two small.

EYE: See Ear.

FEATHER: Pen (Quill).

FENCE: The classification is roughly according to whether the main lines run vertically or horizontally.

FESS: A fess is a band drawn horizontally across an escutcheon or shield, and an embattled fess is one with the top indented as by battlements.

The O for I naturally showed both Shield and Fess, but there is no such drawing. The nearest are listed under Battlements, showing these but no shield.

FINGER: Hand; Thumb.

FISH: Haddock; Whale.

FIREPLACE: Kitchen Range; Bonfire.

FISH SLICE: Trowel.

FLAG: The O for IV showed a Black Flag bearing a very conspicuous White Cross. Flags merely incidental to Boats, etc., not included.

FLAGSTAFF: Not small staffs or poles incidental to Flags.

FLASK: Carafe.

FLEUR-de-LYS: Halberd.

FLOWER: Plant. Those listed as Indt. or unspd. comprise all unidentifiable flowers, whether single or plural, in vases, etc., or not. Considerable care has been taken, with external assistance, over the identification of the classified flowers; but botanical accuracy cannot be guaranteed.

FLY: Dragonfly; Wasp.

FOOT, Winged: Angel. The connection with Mercury as messenger is striking.

FORK, Garden: Trident.

FOUNTAIN: Geyser; Water, jet of.

FROG: Toad.

GALLOWES: The O of VI\* was actually drawn as a Gibbet.

GATE: Door; Turnstile.

GEYSER: Fountain.

GLOVE: Hand.

GOBLET: Brim.

GOD: Angel.

GONDOLA: Boat.

GOWN: Coat.

GRAMOPHONE: Musical Box.

GRAPES: Balloons, bunch of.

GRAVE: Coffin; Corpses.

GRILLE: Portcullis; Trellis.

GUN: Shooting; Pistol.

GYROSCOPE: Spinning Top.

HALBERD: Axe.

HALMA: Chessmen; Pawn.

HAMMER: Mallet.

HAND: Glove; Finger; Thumb.

HARP: Lyre.

HAT: Cap; Cowl. The Hat drawn as O for VI\* appears to be a soft felt, of slightly masculine appearance, but might be feminine. The Hats listed under Fem. var. include two Bonnets (Expts. VI and VII) and one display of Hats in a Milliner's window (VII).

HEADRESS, Red Indian: Shows no Indian.

HEDGE: Fence.

HELIX: Spring; Spiral.

HEDGEHOG: Porcupine.

HOOP: Ring.

HOOR-GLASS: Almost certainly determined in VI by the Orgl. Bow (TIE). Take c and N as 5 and 495.

HOUSE: Building; Doll's House; Hut; Roof; Street.

HUMAN BEINGS: It is impossible to deal with these altogether satisfactorily.

A full analysis by occupation, etc., would take up too much space and would be of little interest. I have accordingly listed separately only the more important, interesting or conspicuous varieties. Nearly all entries refer to single figures, but any may be plural. Some of the special varieties are drawn as 'head only'.

HUMPTY DUMPTY: Egg.

HUT: House, etc.

IGLOO: Hut.

INKPOT: Includes Bottle of Ink, n.s.l. as Bottle.

INSULATOR: As on telegraph pole.

KITCHEN RANGE: Cooker; Fireplace.

KITE: Includes conventional Kite shapes, even if not shown with string, tail, etc.

KNIFE, Table: Includes Bread Knives.

KNOT: Bow; Noose.

LADDER: Steps, household.

LADLE: Spoon.

LAKE: Pond.

LAMB: Sheep.

LAMP: Shades are not separately listed when on Lamps. The Lamp drawn as O for VI was an old-fashioned oil table-lamp with no shade.

LEAF: Cf. Note on Exfoliate.

LOCOMOTIVE: Tractor. Locomotives drawing trains are of course eligible as hits on the O; I have separated them from the others merely as a matter of possible interest.

LOOP: Knot.

LUGGAGE: Bag; Box.

LYRE: Harp.

MALLET: Hammer.

MANDOLIN: Banjo; Violin, etc.

MAPS: Eight of British Isles or part; one each Africa, N. America, Netherlands, Spain and unidentifiable.

MASK, Death: Skull; Horse.

MAT: Rug; Carpet.

MEDAL: Coin.

MILESTONE: Tombstone.

MILK 'CHURN': The sort of container used for transport, not butter-making.

MILL: Windmill; Wheel.

MOSQUE: Building.

MOTORCAR: Side and end views roughly separated only as a matter of interest.

MOTOR HORN: Bugle; Trumpet, etc.

MOUNTAIN or HILL: Volcano. The division into Ordinary and Notably peaked is necessarily somewhat rough.

MOUNTAIN or HILL-SIDE: Gives a few cases where such phrases as "a rough hill-side", etc., occur, or where a marked slope is shown without the top of the hill, etc.

MOUSTACHE: Given for completeness only because O for IV. No solo Moustache is drawn; moustache on faces not listed even if mentioned.

MUG: Cup.

MUMMY: Coffin; Corpses; etc.

MUSICAL BOX: Gramophone.

NAIL, Carpenter's: Includes Tacks.

" Finger; File.

NECKLACE: Includes Strings of Beads.

NET: Merely geometrical reticulations, etc., are not counted.

NIGHTLIGHT: Candle.

NOSE: Noses on faces not counted even if mentioned.

OAR: Paddle. Oars and Sculls purely incidental to Rowing Boats are not counted.

OBELISK: Cromlech; Totem Pole.

ONION: Leek; Turnip.

ORB: Includes circles surmounted by plus-signs.

PADDLE: Oar.

PADLOCK: Lock.

PANTS: Trousers.

PARNASSUS: An early and bad O, unfortunately illd. for II by a Greek Temple at the foot of a Peaked Mountain. No D shows both.

PASTRY BOARD: Drawing Board.

PATH: N.B. Paths, Roads, Rivers and Streams passing over or under Bridges are not listed unless sp. mn.

PAWN: Chess, Halma, Skittle.

PEN, Quill: Feather.

PEPPER-POT: Salt Sprinkler; Cruet.

PICTURE FRAME: Content of Picture, if any, not listed unless sp. mn. or v. consp.

PIER: Quay.

PILLAR BOX: Includes Letter Boxes set flat in walls.

PIN: Nail; Needle; Thorn.

PINCUSHION: Cushion.

PINCERS: Tongs. N.B. Experimenter in VII *drew* Pliers but described them as Pincers. Cf. Note to CORKSCREW.

PIPE, Drain, etc.: Hose.

PISTOL: Includes Revolver or Automatic.

PLANT: Flower.

PLATE or PLATTER: Saucer. Includes Flat Dishes.

POEM: Cases where a Poem, etc., is referred to apart from its contents.

POND: Lake.

PORCUPINE: Hedgehog.

PORTCULLIS: Grille; Trellis.

POST or POLE: E.g., of Gate or Hammock.

POUFFE: Cushion.

PRAWN: No unmistakeable Prawn has been drawn; Lobsters, etc., are nearest.

PUNCH-BALL: Gibbet (shape only).

PUNT: Boat, rowing.

QUAY: Pier.

QUESTION MARK: Only cases where the percipient seems to have thought that a ? was used as the original; not merely to indicate nescience.

QUOTATION: Includes Inscriptions. Refers only to existence of Quotation, etc., as such. Content, if any, separately listed.

RADISH: Carrot.

REEDS: Bamboos.

RIBBON: Bow; Knot; Loop; etc.

RING: Hoop.

RIVER: Canal; Stream. Cf. Note on Path.

ROAD: Path; Tree (Avenue).

ROLLER: Cylinder.

ROOF: Tiles.

RUCKSACK: Bag; Sack.

RUG, Floor: Carpet; Mat. Rug, Travelling; Counterpane, etc.

RULER: Scale; Set Square; Tape measure.

SABOT: Shoe.

SACK: Bag; Rucksack.

SALT SPRINKLER: Pepper-pot; Cruet.

SANDAL: Shoe.

SAUCER: Plate.

SCALE: Ruler. The Scale drawn is of a galvanometer or the like.

SCARF: Shawl.

SCONE: Cake; Loaf.

SCYTHE: Sickle. Many ppts. seem unable to distinguish the two implements.

SHAWL: Scarf.

SHEEP: Lamb.

SHELL: Snail.

SHIP, Steam, War: Not including Submarine, q.v.

SHOE: Boot; Sabot; Sandal.

SHOOTING: Gun. The dictionary word 'Shooting' was illustrated as an O in II by a D of an ordinary Sporting Gun going off. The Expr. thought of drawing a Field Gun, and added a note to this effect, with miniature drawing, at foot of sheet. Field Guns should probably therefore be counted as hits. The only occurrence of the word 'shooting' is in a reference to "the shooting set" by a ppt. of II, while another ppt. of this expt. had an impression of "John Wilkes Booth after he had *shot* President Lincoln." These are the two items listed here.

SIGN: House, public; Picture frame.

SKELETON: Bone; Crossbones; Skull.

SKIPPING ROPE: Loop; Rope.

SKULL: Crossbones; Mask; Skeleton.

SLABS: Grave; Tombstone.

SMOCK: Coat; Dress, etc.

SNAIL: Shell.

SNAKE: Worm; Medusa.

SOFA: Bench.

SPECTACLES: Goggles.



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SPINNING TOP: Gyroscope.  
 SPIRAL: Spring; Swirl. N.B. These are flat Spirals, not Helices.  
 SPIRE: These are cases in which the rest of the Church is not shown.  
 SPOON: Ladle.  
 SPRING: Helix.  
 STAIRS: Steps.  
 STANDARD: Illustrated for II by a Flag with a Lion Rampant. The combination has not yet been drawn.  
 STAR: Solomon's Seal: The 11 instances of I-V are probably best ignored as 'geometrical' items excluded by later instructions.  
 STATUE: Includes Busts.  
 STEPS: Stairs.  
 STOP-COCK: Water, Jet of.  
 STOP-SIGN: Beacon; Policeman; Signal; Traffic Light; Winning Post. Includes (VI) Hand with word 'Stop.'  
 STRAP: Belt.  
 STREAM: Canal; River.  
 STREET: House (pl.)  
 STRIPES: Tiger; Zebra.  
 SUN: Only when definitely intended for Sun; e.g., circles with radiating lines, lacking description or context, are not counted.  
 SWIRL: Spiral; Splash.  
 SWORD: Dagger.  
 SYLPH: Angel, etc.

TABLECLOTH: Counterpane, Rug, etc.  
 TAPE MEASURE: Ruler; Scale.  
 TARGET: Not mere concentric circles.  
 TENT: Wigwam.  
 THORN: Needle; Pin.  
 THRONE: Chair, ceremonial.  
 THUMB: Finger; Hand.  
 TIE: Bow; Knot.  
 TILES: Chessboard.  
 TOAD: Frog.  
 TOMBSTONE: Coffin; Grave; Milestone.  
 TONGS: Pincers.  
 TOWEL: Scarf, etc.  
 TOWER: Chimney; Lighthouse; Pylon.  
 TRAFFIC LIGHT: Stop-sign, etc.  
 TRAMCAR: Bus.  
 TRAY, Household: Includes a flat Meat Tin.  
 TREE: Branch; Stump; Trunk; Wood. The O for V represented a conventional and somewhat 'cabbagy' Tree, which might have been Chestnut, Oak or Sycamore, but not any of the others; I have bracketed the first four classes accordingly.  
 TREFOIL: Ace of Clubs.  
 TRELLIS: Grille; Portcullis.  
 TRENCH: Ditch.  
 TRICYCLE: Bicycle; Ice-cream cart.  
 TRIDENT: Fork.  
 TROUSERS: Pants.  
 TRUMPET: Bugle; Clarinet; Horn; Saxophone. Includes two coiled Horns (VII) described as Trumpets and three ~ straight or 'heraldic' trumpets, as well as one true Trumpet (VI).  
 TUBE: Hose; Pipe.  
 TURNIP: Onion.

VASE: The Amphorae of I-V may have been partly determined by the EWER or JUG used as an Orgl. in V. Take *c* and *N* as 4 and 491 for safety.

- VIADUCT: Bridge.  
VIOLIN: Banjo; Mandolin, etc. Includes Cello, Double Bass, Viola, etc., if any.  
WALL: Not of Houses, etc.  
WASP: Bee; Fly.  
WATCH: Clock.  
WATER, Outdoor: Lake; Pond; River; Sea; Stream.  
WATER, Jet of: Fountain; Stop-cock.  
WATERFALL: Weir.  
WAVES: Not mere wavy lines or incidental waves on Sea.  
WEATHERCOCK: Not if merely incidental to Church; only if Sp. Mn. or v. consp.  
WEIR: Waterfall.  
WELL-HEAD: One in VI has beam and counterpoise instead of winch.  
WHEEL: Spinning Wheel.  
WHEELBARROW: Barrow.  
WIGWAM: Tent.  
WINDMILL: Mill.  
WINDOW: Not mere rectangles with cross lines. WINDOW, Shop: Stall.  
WINEGLASS: Egg-cup. The first class were judged to be Wineglasses but might possibly be Egg-cups; the second class, by shape or context could not be.  
WINNING POST: Stop-sign; Traffic Light, etc.  
WIRELESS SET: Gramophone.  
WORM: Caterpillar; Snake.  
WREATH: Crown.

## Appendix II. a

UNIT SCORES for N = 741

Hits				Misses		Hits				Misses	
c	U	Vce	-U'	c		c	U	Vce	-U'	c	
0	2.867	.0111	.00387	1		53	1.055	.0873	.08282	54	
1	2.562	.0177	.00692	2		54	1.046	.0876	.08376	55	
2	2.384	.0231	.00968	3		57	1.020	.0883	.08653	58	
3	2.256	.0276	.01223	4		58	1.012	.0885	.08743	59	
4	2.157	.0316	.01463	5		59	1.004	.0887	.08832	60	
5	2.075	.0351	.01692	6		63	.972	.0892	.09179	64	
6	2.006	.0383	.01911	7		64	.964	.0893	.09264	65	
7	1.946	.0413	.02121	8		65	.957	.0895	.09348	66	
8	1.893	.0440	.02324	9		66	.950	.0896	.09430	67	
9	1.845	.0465	.02521	10		67	.943	.0897	.09512	68	
10	1.802	.0489	.02711	11		72	.908	.0900	.09908	73	
11	1.762	.0510	.02897	12		73	.901	.0900	.09985	74	
12	1.726	.0531	.03077	13		76	.882	.0900	.10210	77	
13	1.692	.0550	.03253	14		77	.875	.0900	.10284	78	
14	1.660	.0569	.03425	15		78	.869	.0900	.10357	79	
15	1.630	.0586	.03593	16		79	.863	.0900	.10429	80	
16	1.602	.0602	.03757	17		80	.857	.0900	.10500	81	
17	1.576	.0617	.03918	18		81	.851	.0900	.10572	82	
18	1.551	.0632	.04075	19		82	.845	.0899	.10641	83	
19	1.527	.0646	.04230	20		83	.839	.0899	.10711	84	
20	1.504	.0559	.04382	21		84	.833	.0898	.10780	85	
21	1.483	.0672	.04531	22		85	.827	.0897	.10847	86	
22	1.462	.0684	.04677	23		86	.822	.0897	.10915	87	
23	1.442	.0695	.04820	24		95	.773	.0888	.11490	96	
24	1.423	.0706	.04961	25		96	.768	.0887	.11551	97	
25	1.404	.0716	.05100	26		97	.763	.0886	.11612	98	
26	1.387	.0726	.05236	27		100	.748	.0882	.11789	101	
27	1.369	.0735	.05371	28		101	.743	.0880	.11847	102	
28	1.353	.0745	.05503	29		102	.739	.0880	.11905	103	
29	1.337	.0753	.05633	30		103	.734	.0878	.11961	104	
30	1.321	.0761	.05761	31		104	.729	.0876	.12017	105	
31	1.306	.0769	.05888	32		105	.724	.0874	.12073	106	
32	1.292	.0777	.06012	33		106	.720	.0873	.12128	107	
33	1.278	.0784	.06135	34		107	.715	.0871	.12183	108	
34	1.264	.0791	.06256	35		108	.711	.0870	.12237	109	
35	1.250	.0797	.06376	36		114	.684	.0858	.12549	115	
36	1.237	.0803	.06493	37		115	.680	.0857	.12599	116	
37	1.225	.0810	.06610	38		118	.667	.0850	.12748	119	
38	1.212	.0815	.06724	39		119	.663	.0848	.12796	120	
39	1.200	.0820	.06837	40		120	.659	.0846	.12844	121	
40	1.188	.0826	.06949	41		121	.655	.0844	.12891	122	
41	1.177	.0831	.07060	42		141	.581	.0798	.13743	142	
42	1.165	.0835	.07168	43		142	.577	.0795	.13782	143	
43	1.154	.0840	.07275	44		162	.514	.0743	.14459	163	
44	1.143	.0844	.07382	45		163	.511	.0740	.14490	164	
45	1.133	.0848	.07487	46		170	.491	.0721	.14689	171	
46	1.122	.0852	.07590	47		171	.488	.0718	.14717	172	
47	1.112	.0855	.07693	48		213	.384	.0598	.15574	214	
48	1.102	.0859	.07794	49		214	.382	.0595	.15589	215	
49	1.092	.0862	.07893	50		299	.234	.0372	.15902	300	
50	1.083	.0866	.07992	51		300	.233	.0370	.15894	301	

## Appendix II. b

UNIT SCORES FOR N = 491

Hits				Misses		Hits				Misses	
c	U	Vce	-U'	c		c	U	Vce	-U'	c	
0	2.687	.0147	.00547	1		25	1.210	.0816	.06748	26	
1	2.381	.0231	.00972	2		26	1.191	.0824	.06918	27	
2	2.201	.0297	.01351	3		27	1.174	.0832	.07084	28	
3	2.073	.0352	.01699	4		28	1.157	.0839	.07248	29	
4	1.973	.0400	.02025	5		29	1.141	.0845	.07408	30	
5	1.890	.0441	.02334	6		30	1.125	.0851	.07565	31	
6	1.821	.0479	.02628	7		33	1.080	.0866	.08020	34	
7	1.760	.0512	.02909	8		34	1.066	.0870	.08166	35	
8	1.706	.0542	.03179	9		36	1.039	.0878	.08451	37	
9	1.658	.0570	.03439	10		37	1.026	.0881	.08590	38	
10	1.614	.0596	.03690	11		49	.892	.0900	.10091	50	
11	1.573	.0619	.03934	12		50	.882	.0900	.10204	51	
12	1.536	.0640	.04170	13		55	.836	.0898	.10742	56	
13	1.502	.0661	.04399	14		56	.828	.0898	.10845	57	
14	1.470	.0679	.04622	15		71	.713	.0871	.12215	72	
15	1.440	.0697	.04839	16		72	.706	.0868	.12296	73	
16	1.411	.0713	.05050	17		89	.603	.0814	.13496	90	
17	1.384	.0727	.05256	18		90	.597	.0809	.13557	91	
18	1.359	.0742	.05457	19		91	.592	.0806	.13617	92	
19	1.334	.0754	.05654	20		92	.587	.0803	.13676	93	
20	1.311	.0767	.05847	21		93	.582	.0799	.13735	94	
21	1.289	.0778	.06035	22		94	.576	.0794	.13792	95	
22	1.268	.0789	.06219	23		119	.463	.0692	.14946	120	
23	1.248	.0798	.06399	24		120	.459	.0688	.14982	121	
24	1.228	.0807	.06576	25							

## Appendix II. c

UNIT SCORES for N = 495

Hits				Misses		Hits				Misses	
c	U	Vce	-U'	c		c	U	Vce	-U'	c	
0	2.690	.0146	.00543	1		21	1.293	.0776	.06002	22	
1	2.385	.0232	.00965	2		29	1.145	.0844	.07369	30	
2	2.205	.0296	.01342	3		30	1.129	.0850	.07526	31	
3	2.077	.0351	.01688	4		35	1.057	.0874	.08268	36	
4	1.976	.0398	.02103	5		36	1.043	.0877	.08409	37	
5	1.894	.0439	.02319	6		48	.906	.0900	.09931	49	
6	1.824	.0476	.02611	7		49	.896	.0900	.10045	50	
7	1.763	.0510	.02891	8		50	.886	.0900	.10158	51	
8	1.710	.0540	.03160	9		66	.752	.0883	.11744	67	
9	1.661	.0568	.03419	10		67	.745	.0881	.11831	68	
10	1.617	.0593	.03668	11		92	.591	.0806	.13631	93	
11	1.577	.0616	.03910	12		93	.585	.0801	.13691	94	
12	1.540	.0638	.04145	13		94	.580	.0797	.13748	95	
13	1.506	.0658	.04373	14		95	.575	.0794	.13804	96	
14	1.473	.0677	.04595	15		96	.570	.0790	.13860	97	
15	1.443	.0694	.04811	16		97	.565	.0786	.13916	98	
16	1.415	.0710	.05021	17		98	.560	.0732	.13970	99	
17	1.388	.0725	.05226	18		99	.555	.0778	.14022	100	
18	1.362	.0739	.05427	19		107	.518	.0747	.14417	108	
19	1.338	.0752	.05623	20		108	.513	.0742	.14462	109	
20	1.315	.0765	.05814	21							

## Appendix II. d

UNIT SCORES FOR N = 496

Hits			Misses		Hits			Misses	
c	U	Vce	-U'	c	c	U	Vce	-U'	c
0	2.691	.0146	.00543	1	26	1.196	.0822	.06872	27
1	2.385	.0230	.00964	2	27	1.179	.0830	.07038	28
2	2.206	.0296	.01340	3	28	1.162	.0837	.07200	29
3	2.077	.0350	.01685	4	29	1.146	.0843	.07360	30
4	1.977	.0397	.02009	5	30	1.130	.0849	.07516	31
5	1.895	.0439	.02316	6	31	1.115	.0855	.07670	32
6	1.825	.0476	.02608	7	32	1.100	.0860	.07821	33
7	1.764	.0509	.02887	8	33	1.085	.0865	.07969	34
8	1.711	.0540	.03155	9	34	1.071	.0869	.08115	35
9	1.662	.0567	.03413	10	35	1.058	.0874	.08258	36
10	1.618	.0593	.03663	11	36	1.044	.0877	.08399	37
11	1.578	.0516	.03905	12	37	1.031	.0880	.08537	38
12	1.541	.0638	.04139	13	38	1.019	.0882	.08673	39
13	1.507	.0658	.04367	14	39	1.006	.0886	.08807	40
14	1.474	.0676	.04588	15	40	.994	.0889	.08939	41
15	1.444	.0694	.04804	16	41	.983	.0891	.09069	42
16	1.416	.0710	.05014	17	42	.971	.0893	.09196	43
17	1.389	.0725	.05219	18	43	.960	.0895	.09321	44
18	1.363	.0739	.05419	19	44	.948	.0896	.09445	45
19	1.339	.0752	.05615	20	45	.938	.0897	.09566	46
20	1.316	.0764	.05807	21	46	.927	.0898	.09686	47
21	1.294	.0776	.05994	22	47	.917	.0899	.09805	48
22	1.273	.0786	.06177	23	48	.907	.0900	.09920	49
23	1.253	.0796	.06356	24	49	.897	.0900	.10034	50
24	1.233	.0805	.06532	25	50	.887	.0900	.10147	51
25	1.214	.0814	.06704	26					

## Statement of Method for the Non-Mathematical Reader

For many years experiments in telepathy have made use of drawings upon which the agent concentrates and which the distant percipient tries to reproduce. Many such experiments have seemed to be successful because of numerous instances in which the percipient's drawing closely resembled the object or drawing used by the agent.

But there are several grave difficulties in attempting to assess the value of such results. First, the agent may have drawn a commonplace object, or an object frequently thought of. Secondly, though the object be rare, there may be "parallel mental habits" shared by agent and percipient, so that as one follows a certain line of thought the other follows a similar line. Third, even if it could be shown beyond reasonable doubt that such an experiment as a whole is a success, one can never say just *how successful* it is. As one attempts to get control of such elusive phenomena, using a variety of methods, he may properly enough say that all the scores have been high, but whether *one of his methods is better* than the rest cannot be determined without a rather precise estimate of the degree to which the percipient's drawings actually resemble those of the agent.

The present procedure is an attempt to meet all these objections and to propose a simple and repeatable method which will, by and large, bring out clearly the degree of success achieved when free drawings are used. The first step of the present method is to ascertain from a large amount of existing experimental material just how commonly each object is actually drawn by a large number of percipients in a large number of experiments. We may find that there are, in the whole material, 281 lions, 2 zebras and no ant-eaters. In any new experiment in which a drawing of an animal by the percipient coincides with the agent's use of a drawing of an animal, we may proceed to an exact assessment of the value of a hit. If the agent and percipient both give zebra the percipient gets very much more "credit" than if agent and percipient both give lion.

The *Catalogue* presented in this book is nothing more than a list of all the objects drawn by percipients, showing how frequently each one is drawn. Now if chance only is at work the percipient may put down zebra at *any* point in the course of the experiment whether zebra is being actually used by the agent or not. Similarly with lion and the rest. If chance alone is at work, the percipient should be no more likely to call zebra when there really is a zebra than to call zebra when the agent is drawing a lion, an ant-eater, a telephone or a ship.

*Catalogue-scoring* is a matter of counting up all the hits when the appropriate original was actually used on that occasion, and comparing this with all the correspondences between *percipient's drawing* and *drawing made by the agent on some other occasion*.

In the long run, if telepathy is at work, the correspondence of drawings which are *supposed* to correspond will significantly exceed the correspondence of a percipient's drawing with the drawing made by the agent on some *other* occasion.

The scoring of a mass of data consists, then, in giving credit for hits and ascertaining whether they significantly exceed pseudo-hits, that is, hits made on *inappropriate originals*. To achieve this the most important thing to know is exactly how much credit to give for each true hit; for some hits, as we have seen, are worth much more than others because the objects dealt with are rare, and a few really brilliant ones may be worth more than many slight ones.

Next we ask ourselves whether the value of a hit should go up in *direct* proportion to the rarity of the object drawn. At first sight it might seem so; but let us look at it this way: Suppose I am asked to determine clairvoyantly whether each of three pennies is lying heads up or tails up in a safe. This involves exercise of the clairvoyant faculty in three instances. To do it clairvoyantly three times means three times as many clairvoyant acts as if there were one. If, however, I were to state the value of three correct guesses in terms of *chance coincidence*, I should merely say that I have one chance in two of getting the first right; I have one chance in four of getting the first two right; I have one chance in eight of getting all three right (in other words once in two to the third power). The amount of clairvoyance goes up in equal arithmetical steps, 1, 2, 3, 4, etc., while the same *degree of success* would be expressed by stating the chances geometrically, 1, 2, 4, 8, etc. So for any situation the amount of clairvoyance present goes up not in direct proportion to the rarity of the object, but in proportion to the power to which one must raise the original fraction, such as, in the case described, one-half, one-half squared, one-half cubed;  $\frac{1}{2}$ ,  $\frac{1}{4}$ ,  $\frac{1}{8}$ , etc. If the chance of getting something is one in ten, and I make two successes in two such attempts, the result is not ten times as good as one success; as a measure of my clairvoyance it is only twice as good. So if I make one shot when odds are one in ten, one shot when they are one in a hundred, and one when they are one in a thousand, my respective scores are in the ratio 1, 2, and 3.

Now this is all there is to the matter of using logarithms throughout the paper. Logarithms are the simple standard way in which one specifies the *power to which a number must be raised* to give another number. In the formulae one will notice that the Fisher scores are always stated in terms of logarithms.

We are now ready for the formula (*cf.* page 10).

The *number of hits* is written *h*. We know what the chance of making a hit is, because we have the Catalogue at hand. The chance of making a hit is written *p*. We know how many *attempts* were made, having this degree of likelihood of being successful. This number is called *n*. In *n* attempts, we should pile up *n* times *p* successes, or *np*. We now want to find the *excess of this* beyond



the number to be expected by chance, so subtracting we write  $h - np$ .

We now put in the logarithmic expression which will measure the *amount of credit* which each such hit has earned. A moment's reflection will show that the smaller the chance ( $p$ ) of getting a hit the greater the credit should be, so that to assign credit,  $p$  must be in the denominator of a fraction; and this will explain why instead of taking the logarithm of  $p$  we take the logarithm of one divided by  $p$ . The smaller the  $p$ , the greater the fraction  $\frac{1}{p}$  will be. Thus when  $p$  is extremely small the logarithm of one over  $p$  will be extremely large.

To restate again in words rather than mathematical symbols, we take the excess of hits over chance expectation, and assign to each hit a credit corresponding to the rareness of the object indicated; but instead of going up directly we go up as the powers of a given number. (What number we take is immaterial; it is customary in mathematics to write logarithms to a base of ten, saying for example that the log of 1000 is three, which means nothing more than that 1000 is  $10 \times 10 \times 10$ , or  $10^3$ .)

One more logical step is needed. We want to know how this "credit" which we have assigned compares with the "credit" which we should have assigned to other items if they had been correct. It will be remembered that every percipient's drawing must be compared with every drawing used by the agent. In other words, as one succeeds in aiming at a given object, one's drawing must not succeed too well on all the other objects. To take a very extreme case as an example, it does me little good to specify *lion* as item No. 2 when that actually is item No. 2, if in drawing cards from a hat the agent should have drawn lion ten times. I want to compare my drawing not only with the intended original, but also with *all the other items* in the agent's list. This computation of correspondences which are not credited as hits appears in the expression "variance." The formula (cf. page 10) again makes use of  $n$  and  $p$ , with both of which we are familiar, and with  $q$  which is simply  $1$  minus  $p$ . What it does is to take account of the total number of shots ( $n$ ) made whether right or wrong, then multiplying both by  $p$  to indicate the chance of guessing it right and  $q$  to indicate the chance of getting it wrong, obtaining  $npq$ ; but since we have thus put the product of *two* chance events into the formula, each of which is expressed by a fraction, we must multiply the logarithm by itself, which gives logarithm squared,  $\log^2$ . The  $\frac{1}{p}$  is the same as before.

What we have done is to add up, throughout the whole experiment, all the "credits," each being in proportion to the logarithm of its rarity, and then to compare this with the credits which would have been earned if they had been *scattered randomly* on a chance basis throughout the items used.

If the credits exceed a certain arbitrary standard (usually a quantity which would be achieved once in 150 times) they are said to be "significant."

GARDNER MURPHY

### Postscript

Writing now (August 1943), more than a year after completing the above paper, I should like to add a few words based on intervening reflection and experience.

I have no doubt at all that the drawings-technique as a whole, using any statistically valid method of assessment, is truly repeatable, in the sense that anyone who cares to do what I have done will obtain substantially the same results; though he may not, of course, if he elects to do something different. That is to say, I should be on safe ground in retorting to any skeptic "If you don't believe me, go and try for yourself"; and if he feels that collecting a few thousand drawings from a few hundred percipients is too much trouble, that is his misfortune rather than my fault.

The method of assessment by Catalogue and Fisher Scores is enormously more convenient and flexible than any system of "blind" judging, depending on identifying "resemblances"; and the Catalogue here presented should prove at least a very useful yard-stick by which the small-scale or casual experimenter can rapidly assess the approximate value of his results, provided his sample of percipients is reasonably comparable with that of the Catalogue. It is necessary, however, that the main conditions of the experiments should be substantially the same as those of my own, notably as regards the use of fresh percipients making very approximately ten shots each at sets of ten originals.

This is a somewhat severe restriction, because it excludes, or at least discourages, the easiest type of work; namely, that in which a small group of percipients takes part in a series of experiments in each of which all or most of them perform. The reason is that, as experience shows, most people tend to reject any impressions they may have of originals which have been used in earlier experiments, even though they may have been explicitly told that these may be used again in later experiments, as is necessary to maintain the principle of random selection. Consequently, if an original is used a second or third time, there will be a tendency for some percipients not to draw it, who might otherwise have done so, which handicaps the experiment; whereas, if any or all of the percipients were to react in the opposite sense, and draw an object *because* it had already been used, they would not give themselves any advantage. I think

this can be remedied in principle by not telling any of the percipients what any of the originals were until after the whole series of experiments is finished; but not all percipients submit to this, while, even if they do, they will not be, in the second and subsequent experiments, in the same state of "freshness" as the percipients of the Catalogue, very few indeed of whom took part in more than one experiment, and then only after considerable intervals of time.

The fundamental value of the Catalogue, however, lies in the fact that it is henceforward impossible for anyone who performs any kind of experiment with drawings (not diagrams), or for any critic of such an experiment, to say that he has "no idea" of how often the objects concerned would be likely to be drawn under chance conditions. The Catalogue will always enable him to form a pretty good estimate of the relevant frequency.

Moreover, even as it stands, the Catalogue should enable any interested reader to carry out (as I hope some will) investigations of a by no means unimportant character, notably into what attributes of originals (strictly of objects depicted therein) are most favorable to the occurrence of the phenomenon. For example, he may enquire whether originals depicting a household utensil, or beginning with the letter C, or judged to be of an emotion-prompting nature, or commonly met with in daily life, yield significantly higher scores than others not possessed of these attributes. It is the possibility of doing this kind of thing, which arises from the great variety and richness of drawings, that gives the technique, despite its initial cumbrousness, such great advantages as an instrument of research over methods using cards or the like.

WHATELY CARINGTON

